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HEREDITY[1]

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THE conservation movement now in progress has for its end to preserve for future generations of men the natural resources of the earth. But it goes without saying that the movement is useless unless there are to be future generations of men capable of utilizing those resources. Thoughtful persons are beginning to wonder whether this is assured. Man is the product of two sets of agencies which we summarize in the terms heredity and environment. The question has often been asked which of these is the more important, but with this we need not concern ourselves. Both are indispensable. Seed and soil combined assure a harvest, but if either is lacking no harvest can be expected.

The public is awakening to the importance of providing mankind with a proper environment through the agencies of sanitation, education and good government, and this is well. This assures a suitable soil in which a crop of healthy human beings may develop. But what of the seed? This question has not yet been seriously considered. Only in England has it been more than suggested. There Francis Galton and his associates in the eugenics movement have started an inquiry as to why it is that the average physical condition of the English nation is declining although more and more attention is constantly being given to improving the environment. Likewise in Germany statistics show a steadily declining proportion of the young men fit for military service. There is a suspicion in the minds of many, that these nations are producing the new generation of citizens chiefly from inferior family and racial stocks. If this is so the remedy is

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obvious, though how easy of application remains to be ascertained. Would a farmer expect to have full harvests if each year he saved seed from the poorest yielding plants, or could he hope to secure the best results from his herds by selling or butchering the best stock and keeping only the scrubs? Obviously not, and no more can the civilized nations maintain their present standards of manhood if they follow a like practise.

But before any serious attempt can be made to improve the human race considered as an assemblage of animals possessed of certain desirable physical and intellectual attributes, it is obvious that we must know something about heredity in general, and how in particular each of the desired physical and intellectual attributes is produced. Considerations such as these lend general interest to the study of heredity, a subject which has always been of great practical concern to farmers, and of much theoretical interest to scientists. It is my purpose to review briefly some of the problems which the study of heredity presents, and some of the results obtained from their consideration.

"Like father like son" is a homely proverb which shows how general the recognition is that children resemble their parents. Resemblances to grandparents or ancestors even more remote are also of frequent occurrence, and it is convenient to use the term heredity as including all such resemblances, whether to near or to remote ancestors. The phenomenon of heredity is of course not restricted to human society. Heredity has for the stockman and plant-breeder a well-recognized commercial value, because by a knowledge of its laws he is enabled to produce in greater number or with greater certainty animals or plants of a particular type. Indeed, much of our present knowledge of heredity has been derived from a study of the domesticated animals or of the cultivated plants, and from the same sources we may expect to continue to draw, for here alone have we an unobstructed field for observation and experiment, the indispensable tools of scientific research. Just as the sciences of anatomy, embryology, physiology and pathology progressed but slowly so long as the phenomena of the human body alone were considered, but advanced by leaps and bounds when comparative studies on other animals were undertaken, so concerning heredity in man we have learned and can expect to learn but little from the study of man alone, but much from a study of other animals and of plants and from a comparison of the phenomena in the two cases.

Every new individual arises out of material derived exclusively from its parents. This is the basis of heredity. But it does not follow that the new individual will resemble its parents merely. It may resemble remote ancestors more strongly than either parent. For it represents a combination of materials or of qualities derived from the two parents and it is possible that neither parent may manifest all the

peculiarities which it transmits to the offspring. For the parent is made up of two distinct parts, its own body and the reproductive substance contained within that body, and the two may not be identical in character.

The reproductive substance has been called by Weismann the germ-plasm. He it was who first clearly recognized the fact that the germ-plasm is distinct from the body which contains it, and that the influences which modify the character of the one do not of necessity modify the character of the other. Thus he was able to show experimentally that mutilations of the body, as loss of the tail in mice, are not inherited, and to establish with a considerable degree of certainty the principle that characters acquired by the body as a result of use, disuse or other agencies are not inherited, because they have not affected the constitution of the germ-plasm carried within the body.

Weismann's two principles are of fundamental importance to a right understanding of heredity. They are: (1) That the germ-plasm is independent of the body containing it, or, as Weismann put it, that the germ-plasm is continuous from generation to generation, whereas the body dies, and (2) that acquired characters are not inherited.

The hottest biological discussions of the last twenty years have been waged over these two principles and the contest is by no means ended, but year by year the correctness of Weismann's contentions is more generally admitted.

Common experiences support both principles. Thus the independence or continuity of the germ-plasm has been shown from time prehistoric in the practise of castration upon the domesticated animals or upon man. The germ-plasm is localized in particular organs of the body, the reproductive glands. If these are removed reproduction becomes impossible, though all other functions of the individual persist. Further, it is possible to show experimentally that the germ-plasm

FIG. 1. A YOUNG BLACK GUINEA-PIG, about three weeks old. The ovaries taken from an animal like this were transplanted into the albino shown in Fig. 2.

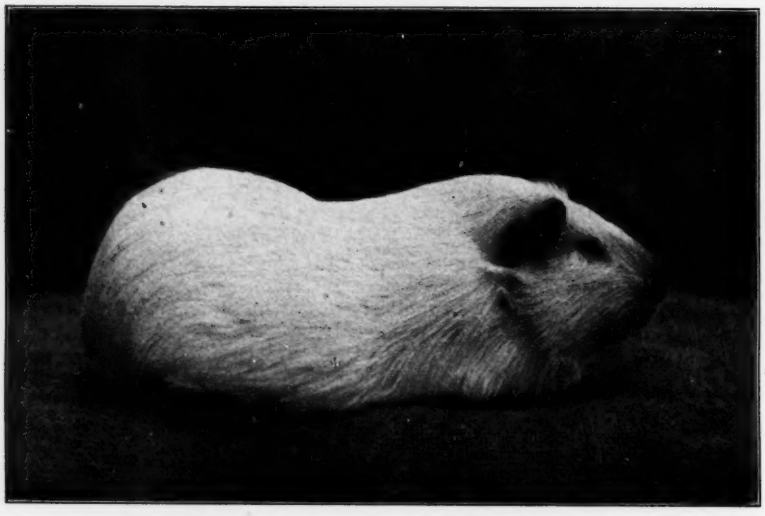

FIG. 2. AN ALBINO FEMALE GUINEA-PIG. Its ovaries were removed and in their place were introduced ovaries from a young black guinea-pig. Compare Fig. 1.

transplanted from one individual into another retains the character which it originally had, quite unaffected by the changed body with which it is associated. This Dr. John C. Phillips and the writer have recently shown in the following way. The ovaries were removed from a young black guinea-pig, Fig. 1, and these were transplanted into the body of a white guinea-pig, previously castrated, Fig. 2. The white guinea-pig was now mated with another white guinea-pig, Fig. 3. Normal white guinea-pigs produce only white offspring when mated with each other, but these two have now produced in three successive litters six young, all black. Three of these are shown in Fig. 4. Evidently the germ-plasm of the black guinea-pig retained its original character even after transplantation into the body of a white one.

In order better to understand the processes of heredity we should be familiar with what takes place when a new individual is formed. The new individual, whether an animal or a plant, has its beginning in the union of two bits of germ-plasm, an egg cell furnished by the mother and a sperm cell furnished by the father. Whether the union of the germ-plasm takes place within the maternal body or not is quite immaterial; among a great many animals it does not.

The new individual, it will be observed, is dual in origin, and to its dying day it retains a dual nature. For the maternal and paternal contributions of germ-plasm retain a certain distinctness as we shall see, and may in part separate from each other at reproduction.

Each germ-cell (egg or sperm), so far as its contribution to heredity is concerned, stands for a complete organism of its species, bears the potentialities of a complete organism, and under appropriate condi-

tions can develop into such an organism. For this idea we have strong experimental evidence. It has long been known that the eggs of certain species of animals can develop without fertilization, *i. e.*, without having united with a sperm or male sex-cell. In such cases there can be no question that the potentialities of an entire organism are contained in the egg, for without any outside help the egg develops into a complete individual of the species. In recent years it has been shown that the eggs of many species in which fertilization normally occurs may by artificial means be made to develop without having united with a sperm. This is true of the eggs of sea-urchins, star-fishes, and of certain worms and mollusks. Such eggs artificially stimulated to development produce entire individuals, similar to those produced by fertilization, but possibly less vigorous.

On the other hand, a sperm cell may be made to develop, if it is allowed to penetrate into a fragment of an egg, even a fragment which lacks the important cell-nucleus. In such cases the entire nuclear material of the embryo is furnished by the sperm, yet the embryo so produced is complete, lacking no essential part, and similar except in size and vigor to normal embryos produced by fertilization.

Accordingly the evidence is fairly complete that each germ-cell (egg or sperm), considered as the vehicle of heredity, represents a complete organism, and that an individual produced by the union of two such germ-cells represents twice over each heritable trait of the species. In other words, the germ-cell is single, the individual is double.

This fundamental principle of the singleness of the germ in con-

FIG. 3. AN ALBINO MALE GUINEA-PIG, with which was mated the albino shown in Fig. 2.

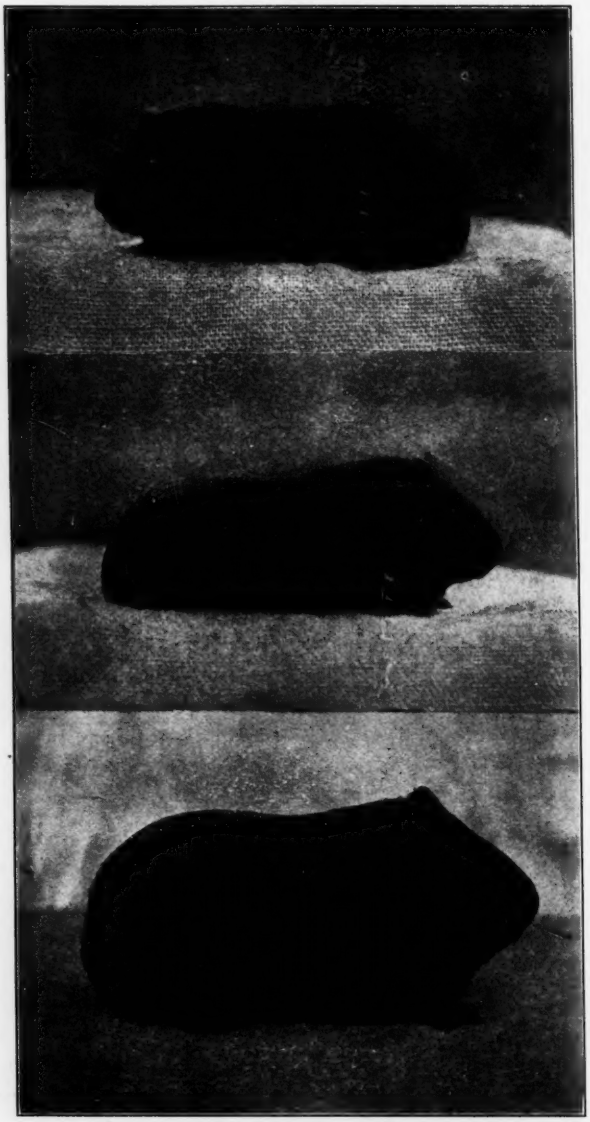

FIG. 4. A GROUP OF THREE YOUNG, produced by the pair of albinos shown in Figs. 2 and 3.

trast to the doubleness (duality) of the individual receives the fullest confirmation from experimental breeding.

If we mate a pure-bred black guinea-pig with a white one, the young are all black pigmented. This result seems to violate the principle previously stated that both parents contribute equally in heredity; in reality, however, that principle is not violated. The white parent has contributed its own character to the offspring, but that contribu-

tion is unseen in them simply because black hides it. The white will reappear among the grandchildren. In Fig. 5 we see a mother guinea-pig having a jet black coat. Beside her are four young of the same color as herself. The father too was black. In a word this black race breeds true. A female of this race was mated with the albino male shown in Fig. 6. Albinos have white hair and pink (unpigmented) eyes, the red eye color being due to the blood which shows through; they breed true among themselves, but the result is very different when they are mated with black individuals. Two children of the albino male and the black female are shown in Fig. 7. They are intensely black pigmented, as are all the young produced by this cross. Two of them when grown to maturity and mated with each other, produced a litter of four young, shown in Fig. 8. Three are black pigmented like the parents, but one is an albino similar in all respects to the albino grandsire. Here we notice the reappearance of the albino character after skipping a generation. The albino grandsire really made a hereditary contribution as regards the character hair color, but it did not show in the children, because black also was present in the children, and black obscured or dominated the white.

Applying our principle of single germ, dual individual to this case, we see that the facts observed are fully in harmony with it. The original cross brought together the characters B (black) and W (white) into an individual (or zygote as we call it, a joining together) B W, which showed only black. Two such individuals, a male and a female were now mated together. In the formation of germ-cells by these individuals there is a return to the single condition, B separates from W and passes into a different germ-cell. Accordingly, the mother forms eggs, B and W, respectively, and the father forms sperms of a like character. Now a new individual arises from a union of an egg with a sperm. Apparently either sort of sperm may unite with either sort of egg which it chances to meet. So there are formed in the next generation three sorts of zygotes (individuals), *viz.*, B B, B W, and W W, instead of B W alone as in the previous generation. The chances for the occurrence of these three sorts of unions are 1 BB, 2 BW, and 1 WW. Any individual containing the character B will be black; accordingly the BWs as well as the BBs will be black and there should be three blacks to one white. These are in fact the observed proportions. The white individual should transmit no other character, because it contains only W. Such is indeed the observed fact. Any two white individuals mated together will produce only white offspring. But, if our reasoning is correct, two thirds of the black individuals of this generation (*viz.*, the BWs) should transmit white as well as black, while the remaining one third, BB, should transmit only black. Experiment justifies both these conclusions. If we mate the black animals

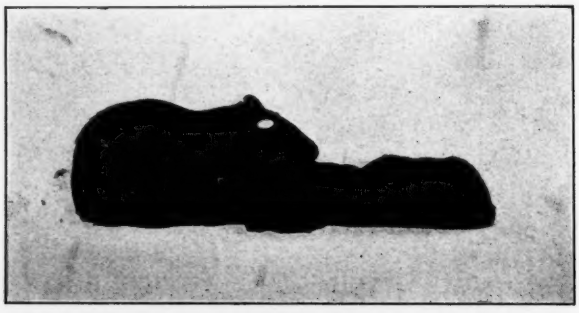

FIG. 5. A BLACK FEMALE GUINEA-PIG AND HER YOUNG.

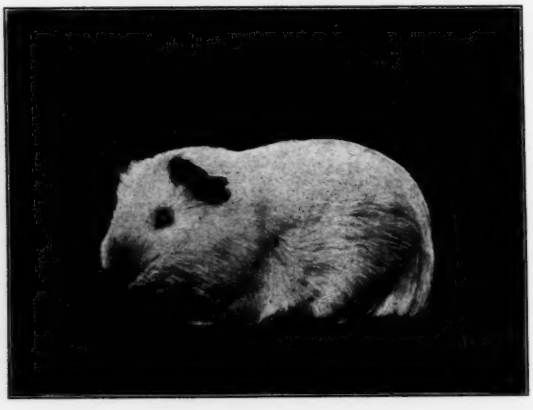

FIG. 6. AN ALBINO GUINEA-PIG, father of black young, like those seen in Fig. 5.

of this generation, one by one, with albinos, we find that on the average two out of three of them will produce white offspring as well as black ones, while the third one produces only black offspring.

The scientific law which governs the inheritance of albinism, and of other characters transmitted in a similar fashion, is known as Mendel's law. It applies, apparently, to all cases of color-inheritance, as well as to the inheritance of characters of many other sorts. Through its operation new combinations of the peculiar characters of individuals or of races can be obtained in the course of one or two generations. Thus when a guinea-pig showing the two coat-characters seen in Fig. 9, dark and smooth coat, is mated with one showing the combination, white and rough, Fig. 10, young are produced showing a wholly new combination, dark and rough, Fig. 11. And if these young are at maturity bred together, a fourth combination, white and smooth, appears among their young, the grandchildren. See Fig. 12. Other grandchildren manifest the combinations seen, respectively, in the parents and in the grandparents. By selection any one of these combinations may be obtained in a pure race.

Oftentimes a new combination of characters obtained through

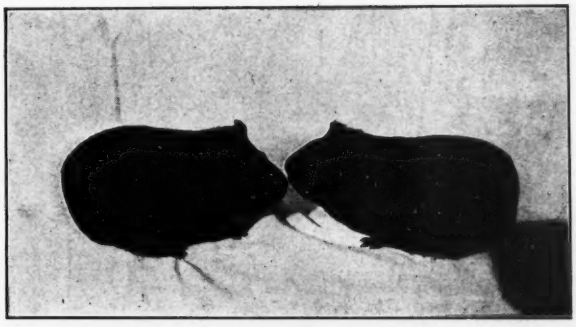

FIG. 7. TWO OF THE GROWN-UP YOUNG OF A BLACK AND OF AN ALBINO GUINEA-PIG.

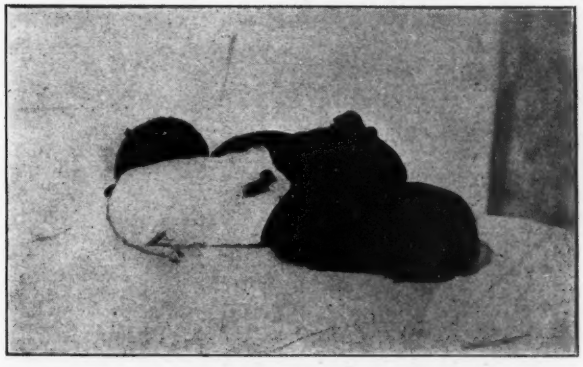

FIG. 8. A GROUP OF FOUR YOUNG, produced by the animals shown in Fig. 7.

crosses coincides with a lost racial combination. Then the phenomenon is called reversion or atavism. Thus when yellow rabbits are crossed with black ones, gray offspring are obtained similar to wild rabbits in coloration. There is no longer anything mysterious about the process; it is simple recombination of different unit-characters formerly associated together in the same race, but since isolated in some of the derived races.

Very different in nature, apparently, from the Mendelian inheritance of unit-characters is the result obtained when races of animals are crossed differing in size or in the proportions of their parts. In such cases the children are intermediate in character, and the grandparental conditions do not reappear among the grandchildren. The result may be described as a blend apparently permanent. Fig. 13 shows the skulls of three rabbits, all adult, father, mother and son. The skull of the son is shown between that of his parents, the mother's skull being at the right. Size and proportions of parts are clearly intermediate in the son. No grandchildren were obtained like either grandparent in size. The color of the coat in this same family of rabbits clearly followed Mendel's law, although the size characters blended. The practical result is that one may at will produce a race

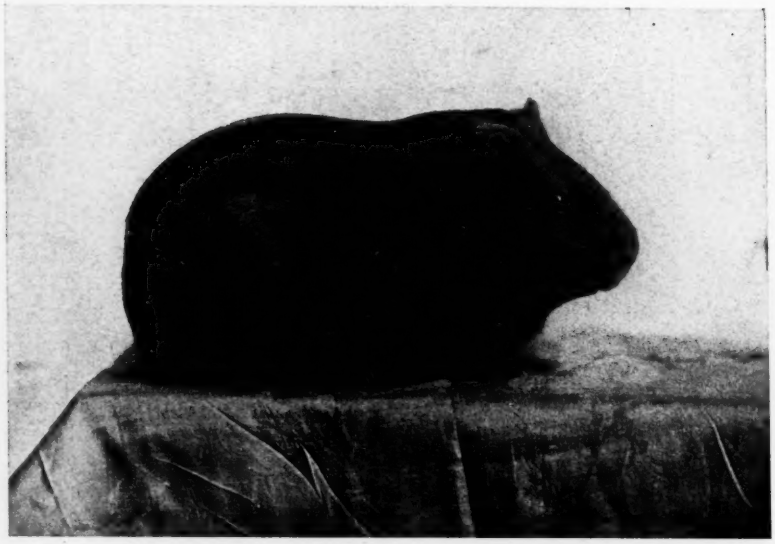

FIG. 9. A *dark smooth* GUINEA-PIG.

of rabbits of any desired size within the known limits of variation in size among rabbits, and with any of the conceivable combinations of color factors. Size variation is apparently continuous and its inheritance blending, color variation is discontinuous and its inheritance Mendelian.

Notwithstanding the seemingly radical difference between these two types of inheritance, it is possible that they may, after all, prove to have

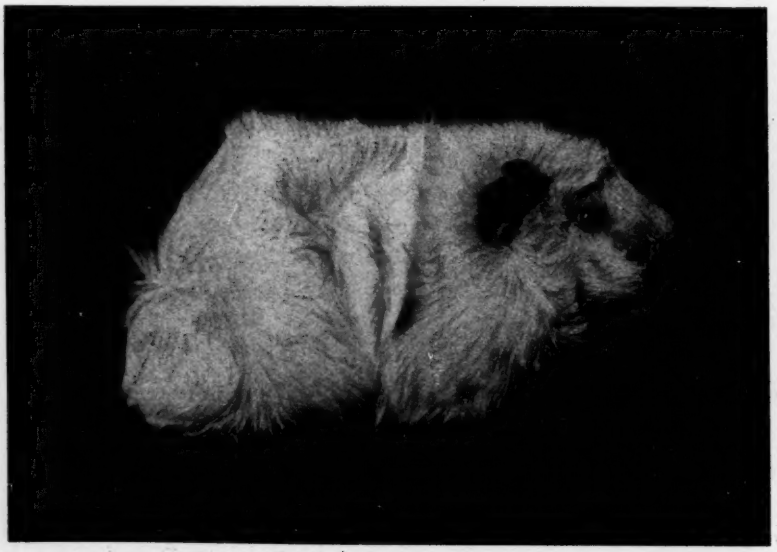

FIG. 10. A *white rough* GUINEA-PIG.

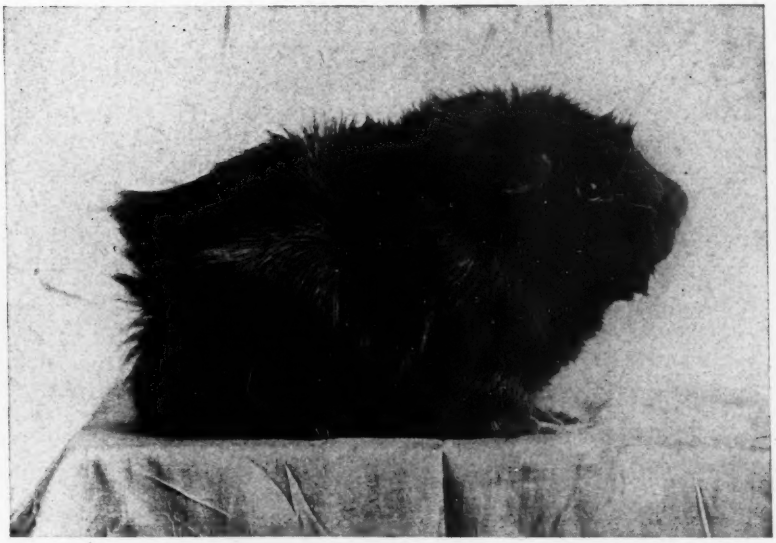

FIG. 11. A *dark rough* GUINEA-PIG, the new combination of characters obtained when animals are mated like those shown in Figs. 9 and 10, respectively.

a common basis. Blending inheritance may possibly be only a complex sort of Mendelian inheritance, in which many independent factors are simultaneously concerned. The question is one of much theoretical interest. Its solution awaits further investigation.

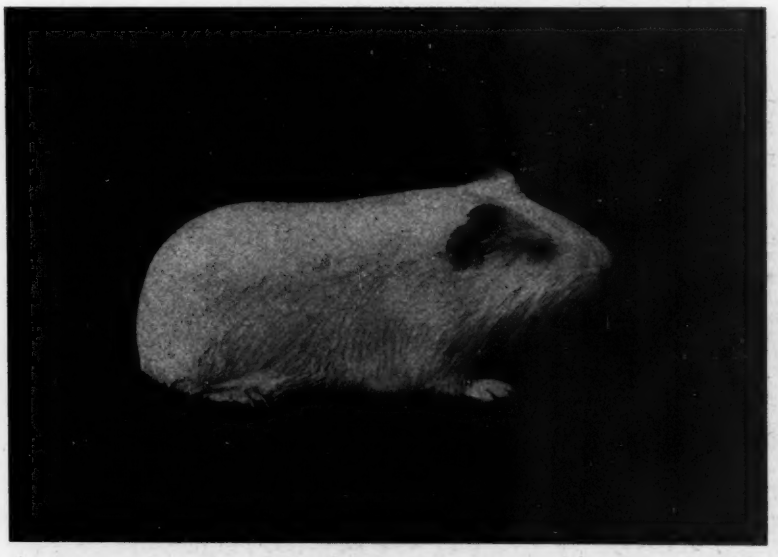

FIG. 12. A *white smooth* GUINEA-PIG, a second new combination of characters, but obtained first among the grandchildren of such animals as are shown in Figs. 9 and 10. Other grandchildren are like the respective grandparents (Figs. 9 and 10) or the parents (Fig. 11).

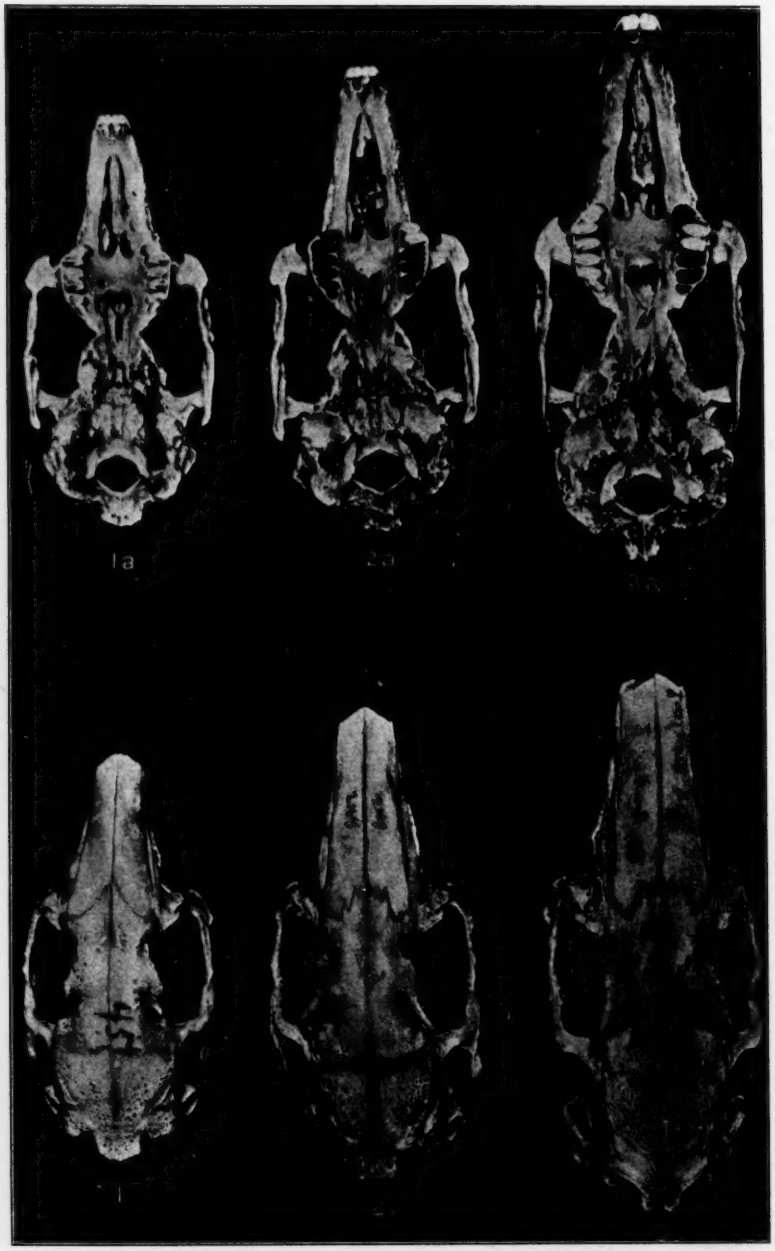

FIG. 13. SKULLS OF THREE RABBITS, mother (3 and 3*a*), father (1 and 1*a*) and son (2 and 2*a*). From Publication No. 114, Carnegie Institution of Washington, by permission.

What has already been accomplished in the study of heredity gives us a hopeful outlook for the future. We are gaining a fuller knowledge of its processes, and a knowledge of processes is a first step toward their control.

SNEEZING, SEA-SICKNESS, PAIN

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PHYSIOLOGISTS can not lay claim to a theory of pain. Even definition is difficult. The distress of bodily wants overlaps pain on the one side; fear, anxiety and similar mental states overlap it on the other. Excessive stimulation of certain organs of special sense, particularly those for touch, temperature and hearing, leads up to it. If an attempt be made to isolate, in thought, the effect in consciousness to which the term "pain" properly applies, it may be said to be the awareness of something amiss in some part of the body, irrespective of the testimony of either of the special senses. It is a modification of consciousness, not a part of its content. We have learned to associate the receipt of the modifying influence through particular nervous channels, with its provenance, just as we have learned to associate sensations of touch conveyed by particular nerve-fibers with the contact with external objects of particular regions of the skin; but such topognosis is no more innate in the one case than in the other. It is the product of self-investigation, and is based either upon the testimony of the eye or upon experiments in moving the hand to the spot. Hence in the case of organs which are out of sight and out of reach topographical guidance is unobtainable. Since we can have no knowledge of its seat, the pain is referred to some accessible part of the segment of the body in which it occurs. A gulp of very hot water, on reaching the closed sphincter muscle of the stomach—the valve which must open before it can pass from gullet into stomach—gives rise to pain which seems to have its seat in the skin over the lower end of the breast-bone. In the same way disease of the various viscera gives rise to pain and tenderness of areas of the skin of the segments of the body in which the nerves of the viscera join the spinal cord.

From a physiological standpoint "pain" and "sensation" are antithetical terms. Sensations inform. Pain is a state of consciousness which masks sensation. Sensations are transient. Their apparent prolongation is due to repetition. They are vibratory. Pain is a condition, slowly set up, slow to disappear. Even the briefest pain is long as compared with the constituent unit—a nerve wave—of sensation. It is of the very essence of sensation that it has quality or modality; the informing value of any given sensation depends upon its excluding all other forms of stimulation. The sensation of a bright red spot of light is not susceptible of confusion in place or quality with other visual sensations. Still less is it liable to be mistaken for a sensation of hearing or of taste. Pain has no modality. If it may be

justly described as stabbing, aching, burning, it owes its individual character to the form of its onset and to its duration, and these in turn depend upon either the vascular condition of the part affected—the pumping of blood through the vessels of a tissue free to expand, or packed in a bony case—or they are due to the effect upon the inflamed or injured part of muscular contractions. If the injured part be inaccessible, pain has no "local sign." If it be on or near the surface of the body the pain felt in it has or seems to have a topographical meaning; but it is very doubtful whether the mind can localize the source of pain in the absence of evidence simultaneously afforded by the nervous apparatus of the sense of touch. Many instances are on record of disease or injury to the central nervous system resulting in complete loss of sensitiveness to pain, whilst sensitiveness to touch and pressure remained undiminished. But there are no recorded cases, so far as we are aware, of complete paralysis of the mechanisms of touch and of the recognition of heat, of cold and of pressure, with the retention of normal sensitiveness to pain. Such a condition, if it were established, would make it possible for an investigator to ascertain whether skin-pain, by itself and unsupported by collateral evidence, has a topographical meaning, or "local sign"; and whether the expression pain-spot may be legitimately used, as meaning a sounding spot in the midst of a dumb area, and not merely a focus of sensitiveness at which the weakest stimulus which can evoke pain is effective.

Dr. Henry Head caused the large cutaneous nerve of the thumb-side of the forearm and hand to be cut in his own arm, in order that he might study carefully the revival of sensations which follows on nerve repair. He found that, long before he regained the ability to distinguish degrees of warmth, to feel as separate the two points of a pair of compasses, or to recognize a touch with cotton-wool, he regained his power of recognizing stimulation by agents that do harm—hot things, cold things, pricking with a pin—but his power of localizing the spot injured was extremely vague. Trotter and Davies have made similar experiments in their own persons on a still more extensive scale and have confirmed and amplified Head's results.

Investigations with the aid of new histological methods has shown that the epithelial tissues are supplied with nerve-filaments in inconceivable abundance. It is probable that the conclusion is justified that every cell of the skin, of the mucous membranes, of the lining epithelium of the air-chambers in the lungs, of the pleural and peritoneal cavities, of the various glands, is connected with a nervous thread. It is certainly true also of every muscle-fiber in the walls of the alimentary tract, of ducts and of blood-vessels. By these filaments the cells of the body-surfaces both external and internal, the central nervous system and all motile organs, are bound together.

Superimposed on this basal system are the various specialized sys-

tems of nerves which originate in organs in which their ends are so modified and so enveloped as to render them sensitive in the highest degree to one particular order of stimulus, whether of smell, sight, taste, hearing, touch, heat, cold, pressure or traction, and inaccessible to stimuli of every other class. These nerves, with the chains of neurones which link them to the muscles, *via* the spinal cord and brain, stand out as a pattern on the basal system, like the pattern formed of thicker fibers and coarser knots on a sheet of lace.

In a book recently published, "The Body at Work," I have endeavored to present a picture of the nervous system and its activities, which, although not original in any of its details, is new in their grouping and in its comprehensiveness. It is based upon the teaching that the two great functions of the nervous system, notwithstanding that they grade one into the other, must, for purposes of analysis and description, be considered apart. By the basal system of protopathic nerves all the cells of the body, with the exception of those of the connective tissues, bones, tendons and so forth, are bound together into a continuous inseparable whole. No change can occur in the nutritive condition of any part of the skin or of an internal epithelium without the induction of a nutritive change in the central nervous system and thence, onward, in the plain muscle-fibers of arteries and other structures of the segment of the body in which the inducing change occurs. As contrasted with the influence which spreads through this basal system the "impulses" which travel up the nerves of special sense are peculiar in kind or, at any rate, in intensity. In order that they may overcome the resistance of a chain of neurones they have a certain potential, and progress in pulsations or waves.

Pain is explained as due to the setting up in a particular segment of the axial nervous system of a focus "pain-conditioned" sympathetically with the injured tissues. Consciousness of pain depends upon the direction of attention to impulses which ascend through the pain-conditioned segment from end-organs of nerves of special sense. If the seat of injury be the skin it is through the specialized nerves of the injured spot that modified impulses reach the cortex of the brain. If the seat of injury be an internal organ no effect is produced in consciousness until the pain agitation of the spinal cord has become sufficiently intense, and sufficiently wide-spread, to modify impulses which ascend to the cortex from skin areas of the segment in which the viscus is situate. The pain in angina pectoris is felt on the left side of the breast bone at its lower end. This shows that the nerves of the aorta have their centers in the same region of the spinal cord as the cutaneous nerves of this area on the surface of the chest.

To give an illustration of the difference of mechanism of pain and of sensation. In a railway station lavatory I recently observed a man who absent-mindedly placed his fingers on a free-standing iron stove

to ascertain whether it was hot. It was a frosty morning in November. Obviously, the stranger had a strong prejudice that the station-master would not have thought it necessary to order a fire to be lighted so early in the winter. As nearly as I could estimate, three seconds elapsed between the touching of the stove and the ejaculation which announced with unnecessary emphasis that the man had obtained the information he desired. Had he, expecting to find the iron hot, directed his attention to the modification of his skin sensation he would have withdrawn his fingers in one seventh of a second.

One of the characteristics of incipient pain is exaltation of reflex actions. I can not by any effort of will prevent my muscles from withdrawing my hand from hot iron (although the resolute withdrawal of attention from pain-modified sensations and the forcing of a conviction that it does not exist, has in certain cases a remarkable effect in suppressing pain). Equally characteristic of established pain is the inhibition of action. A whitlow abolishes all temptation to shake the finger.

Physiologists can not investigate the phenomena of pain, although they make elaborate studies of its threshold value and of the distribution of "pain spots." It would take us too far were we to consider the evidence of a degree of specialization in the protopathic nerves of the skin which is held by some to justify the use of the expression "pain-nerves," and of the allied question of neuronic conduction of incipient or threshold pain along pain-tracts in the spinal cord.

The chief interest of the hypothesis of structural continuity through the protopathic nervous system, with its corollary of sympathetic nutritional change, lies in the explanation which it affords of the influence upon reflex action of the establishment of a pain-condition in the axial nervous system in circumstances in which, consciousness not being affected, there is no "pain."

Pain-condition which inhibits reflexes due to impulses which start in the damaged organ or skin area, greatly increases in many instances the conductivity of the portion of the nervous system which it affects for impulses which do not come from the damaged part. Such a reinforced reflex is the attack of sneezing to which many persons, most monkeys, and some breeds of dogs are subjected when the eye is stimulated by a bright light. When the gaze is directed towards a bright cloud, excessive stimulation of the retina sets up a pain-condition in the mid-brain. In the progress of evolution this portion of the cerebrospinal axis has undergone great changes. Its sensory nerves with their protopathic constituents have been drawn backwards into the great bundle of the fifth nerve, which joins the hind-brain, whilst the nerve from the retina has established a secondary connection with the mid-brain. The mid-brain receives in consequence the protopathic nerves of the eye. But the nose being the real tip of the body and anterior to the eye the sensory fibers of the skin which lines it al-

though bound up with the fifth nerve, extend forwards within the cerebrospinal axis to the very front of the mid-brain. A remarkable state of affairs is thus established. The mid-brain receives the protopathic nerves of the eye and the root-fibers of the sensory nerves of the nostrils. When excessive stimulation of the retina by bright light sets up a pain-condition in the mid-brain the every-moment impulses ascending from the nostril acquire undue importance. It is as if they had been increased in intensity by a pinch of snuff. Their urgency causes the reflex by which irritating substances are expelled.

Some persons merely feel a tickling in the nose when they look at a bright light, but do not sneeze. This phenomenon is extremely interesting. It proves that stimuli "adequate" to impress nerve-endings are not necessarily "adequate" to arouse consciousness. External forces incessantly press the button with sufficient energy to make contact. At each pressure a bell rings in the chamber of consciousness, but, if it is to attract attention, it must ring more loudly. The stimuli which gave rise to a tickling feeling were not originated nor intensified by the light which fell upon the retina. The mid-brain through which impulses passed to reach the cortex was rendered more conductile.

In normal conditions no pain results from stimulation of the retina, however severe; because the nerve-fibers which convey visual impulses from this highly specialized sense-organ, are connected, not with the mid-brain, but with the optic thalamus and the occipital cortex. It would stultify so highly specialized a sense, were its news admixed with, or modified by any influence or information not directly connected with its proper function.

Sea-sickness is another illustration of the effect upon reflex action of central agitation due to impulses which do not appear in consciousness however voluminous the sensations may be of which they are the indirect cause. The nerve which is concerned with the adjustment of the position of the body is a constituent of the auditory nerve. It comes from the semicircular canals. Never under any circumstances do the impulses which originate in these organs of orientation enter consciousness; but when a ship begins to roll, or worse, to heave, they churn up the gray matter of the hind-brain until its conductivity is so affected as to demonstrate their urgency beyond misunderstanding. Root-fibers of the vagal nerve traverse the hind-brain much in the same way as root-fibers of the fifth nerve traverse the mid-brain. Habitual, every-moment impulses ascending from the stomach by the vagal nerve, for the routine regulation of its purely domestic functions, acquire, when the hind-brain is pain-conditioned by impulses from the semicircular canals, a terrifying import, causing the explosion of numberless motor neurones. The stomach sneezes, with the zealous support of muscles of the throat, chest and abdomen. In the

early stages of the malady the same cause leads, no doubt, to reflex derangement of the secretion of the stomach involving nausea. Disturbances of vision add to the victim's discomposure, and in some small degree precipitate the stomachic catastrophe; but the effective cause is, I take it, the agitation, by impulses from the semicircular canals, of the gray matter of the hind-brain.

Hiccough, again, is an exaggerated reflex due to increased conductivity of gray matter. A child takes a cold drink, or he rapidly fills his stomach with insufficiently masticated food. The ends of the vagal nerve in the stomach are irritated. They convey an influence which sets up the pain-condition in a portion of the gray matter through which nerve-fibers from the lungs extend their roots towards the nucleus of the phrenic nerve. The diaphragm sneezes.

It would carry us beyond the proper sphere of this journal were we to consider the phenomena of inhibition of some reflexes and exaggeration of others which the modification of the normal conductivity of gray matter due to the establishment of pain-conditioned foci, brings about in hysteria, angina pectoris and many other morbid conditions.

The pain of headache is as truly "referred" as is the pain of angina pectoris, although it must be assigned to a different category. Medical men tell their patients that their headaches are in their scalps and not within their skulls. The patient finds it difficult to understand how this can be, when there is nothing the matter with his scalp; but agrees with his doctor that, were it otherwise, it would be impossible to explain the beneficial effect of a cold wet rag. Again as in sea-sickness the vagal nerve is at the bottom of the mischief. Indeed, in many persons, intolerable headache takes the place of sickness on the sea. Impulses ascending the vagus agitate the gray matter of the hind-brain. Into this pain-conditioned gray matter the nerves of the scalp pour a constant stream of impulses. Myriads of fibers connecting the scalp with the brain twang ceaselessly with messages to which, under normal circumstances, consciousness gives no heed—until a draught of cold air or the tickling of a fly's feet accentuates a certain group. Let the gray matter through which they pass be pain-conditioned, the vibrations traveling to the cortex from innumerable spots on the surface of the head produce a widely diffused dull ache which has no sensational quality, because no particular group of nerve-endings is being especially stimulated by external force. Vascular changes in the scalp due to the same cause, the exaggeration of impulses during their transit of the gray matter of the hind-brain, making believe that the scalp is injured and needs more blood, react upon the nerve-endings increasing the illusion of injury. The pain is no illusion. It is impossible to decide whether vascular changes are the first effect of vagal agitation and therefore the immediate cause of pain or whether they are merely subsidiary results of the exaggeration of sensory impulses from

the scalp. A cold compress by constricting the blood-vessels reduces the din of the multitudinous messages shouted into consciousness by the sense-organs of the scalp. If completely successful it subdues their chorus to its habitual murmur, too faint to secure attention. Long continued, and therefore damaging, contraction of the muscles which move the eye-ball, and particularly its elevator, sends through their protopathic nerves a stream of influence to the mid-brain, with which these nerves are connected, which causes frontal headache in just the same way as the influence which ascends the stomach nerve.

Would doctors be more logical if they said that the headache was in the hind-brain—the region which contains the agitated gray matter which gives pain-value to the impulses from the scalp—or if they said it was in the stomach? The irritation of nerve-endings in the stomach is the origin of the trouble, seeing that it sets up the pain-condition in the hind-brain.

Psychologists base their science upon conspicuous sensations—sensations of sufficient prominence to stand out in the field of consciousness. They can do no otherwise. But the terminology in which they have expressed the results of their analysis hampers physiology. In the process of conduction a physiologist can distinguish no stages intermediate between stimulation and muscular response. In reactions to which consciousness is adjunct, as judged by self-feeling, or, when outside oneself by attribution of self-feeling, the nerve-current may be termed a sensation, and sensations may or may not provoke attention. Nothing is gained by classifying the sequence of events into stimulation, passage of impulse, sensation, perception. Such terms are machinomorphic. The nervous mechanism is infinitely vibrating. "I always hear my clock stop" can have but one meaning. Every tick of the clock produces an answering vibration of the auditory nerve, however little attention be given to the message; and attention carries with it the idea of something which attends. Pain, as pictured in this essay, is the interpretation which the ego gives of hitherto unperceived sensations when they are increased in volume without definition. Pain is developed when impulses, without informing attributes, are raised in urgency to the level of attention.

The passage of a gall-stone from the gall-bladder to the intestine is the cause of intense pain, "referred," in the first instance, to the skin which overlies the liver. Yet the gall-bladder is insensitive. As surgeons have long been aware, the liver, stomach and other viscera may be cut, burned, scarified, without arousing pain. Laying stress on these two well-known facts, (1) the insensitiveness of the viscera and (2) their liability to become the source of referred pains, James Mackenzie has defined pain as "a disagreeable sensation due to stimulation of some portion of the cerebrospinal nervous system and referred to the peripheral distribution in the body wall of cerebrospinal sensory

nerves." When a viscus is the seat of origin of pain the impulses which ascend its sympathetic nerves excite the centers of sensory nerves in the spinal cord.

The theory which I have attempted to outline in this article is laid on the same basis, somewhat broadened. All pain is "referred"—to the right spot, if its source be in the skin; because the skin is elaborately supplied with place-defining nerves—to an organ or part, skin, muscle, joint, which the ego, during the progress of self-investigation, has discovered in the same segment of the body, if its source be in a viscus.

The body is permeated with a felt-work of nerves, unprovided with specialized nerve-endings, conveying no definite information, and in consequence without precise distribution in the seat of consciousness. This non-specialized system which binds the various parts of the body together is the mechanism through which the caliber of blood-vessels, erection of hairs, secretion of glands, contraction of the walls of ducts and of the intestines, and many other domestic adjustments are effected. It is also the medium through which the gray matter of the cerebro-spinal axis is affected sympathetically with damage to the tissues. The resultant altered conductivity of the gray matter leads to modification of the only kind of impulses with which consciousness is concerned—impulses which inform. We infer that the damage which is giving rise to a feeling of pain is in the part from which the modified impulses come.

When attempting to formulate the theory of pain it is necessary to discard the prejudice that there need be a proportional relation between the intensity of pain and the magnitude of the physiological changes which condition it. A heavy blow hurts more than a light one. Yet a change which could not be detected by any piece of apparatus in use in a physiological laboratory, if it affect the nerve-tissue of a tooth, may give rise to more pain than is caused by a crushed limb.

Another prejudice, from which it is difficult to shake free, attributes to the mind an innate knowledge of the topography of the body; an innate knowledge, that is to say, of the distribution of its news-agents, the sensory endings of nerves.

Thirdly, it is necessary to remember, when investigating the machine, that the machine is the man. It is not sufficient to design a scheme of telephone wires requiring for its use a listening ear at its center, the brain. The ear is a part of the machine. There is no need to picture a system of pain nerves, carrying news of damage to an attentive mind. A departure from the normal in the functioning of the sensory apparatus is pain.

THE CIRCULATIONS OF THE ATMOSPHERES OF THE EARTH AND OF THE SUN

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The Two Causes of Circulation on a Non-Rotating Earth

Gravity, Temperature and Pressure

IT must seem rather ambitious to attempt to treat so great a subject as that of the circulation of the atmospheres of the earth and the sun in a single lecture. It is true that if it should be discussed fully, in the technical way, it would require a great many lectures, but of course there are at the same time certain fundamental principles which are common to all circulations that can easily be studied, and then illustrated by the known facts of the circulation in these two atmospheres. All circulation depends upon two primary causes, the first being the attraction of gravitation, by the laws of the action of the earth upon its atmosphere or the great body of the sun upon its atmosphere; and, secondly, the difference of temperature which exists in different parts of a given atmosphere. If we had an earth standing still in space without rotation upon its axis and the sun were withdrawn for a considerable time, the atmosphere of the earth would gradually settle down into a quiescent state, which may be described as consisting of a series of concentric shells, each shell having a certain fixed temperature passing around the earth at the same distance from a center, as if a balloon were floating at the same height above the surface, where will be found the same barometric pressure and the same temperature in all latitudes and longitudes. If the balloon falls from one shell to another it would pass into layers of greater density, and if it rises, into layers of less density. The boundary of each shell may be conceived as a surface having the same force of gravity acting upon it, and this is called the gravity level. In this case the surfaces of equal pressure or the isobars, and the surfaces of equal temperatures, isotherms, both coincide with their own gravity levels. Everything is quiescent and there is no circulation. It is quite important to secure a clear idea of the fact that the isobars, isotherms and gravity levels coincide wherever the layers in the atmosphere have the same temperature. As a matter of fact the earth is not without rotation, and the sun is shining upon it, sending enormous masses of heat which fall upon the tropics, and it is our problem to study the effect of this heat, at certain layers in the earth's atmosphere, upon the circulation of the entire mass.

To illustrate the series of causes and effects we can take a long box or canal containing air at a certain temperature. Now if heat be applied at one end, it is evident that the air at that end is displaced in proportion to the amount of heat. The effect of heating the bottom of a column of air is to expand the lower layers of it and this produces less density in each of the lower layers, while at the same time the entire mass is lifted, provided the bottom rests upon a solid surface. Take water in a tube, heat the lower part of the tube, and the whole column will seem to rise in the tube, but the lower parts, being hotter, will necessarily have a smaller density. A common case of the power that can be produced by heat is seen in its effects in the steam engine. Similarly, the air when heated in certain localities, as over the tropics, begins to work practically like the steam engine. The air is expanded, the upper part is elevated and the lower part is rarefied. Now the effect of lifting a column which is heated in the lower part is to raise the isobar above the gravity level which is occupied before heating, and in the lower part the isobar is depressed below the position which it had before it was heated. It is now readily seen that isobars, instead of coinciding with the gravity levels, have a slope, the upper ones trending downwards towards the cold end of the canal, and the lower one sloping downwards to the warm end of the canal. Under the action of gravity a liquid or a fluid which rests on a slope of any kind tends to run down hill, just like water in a brook or a railroad train on a grade. The part which is above the gravity levels tends to get down to it, in order to destroy the slopes which nature abhors among its gravity levels. The force of gravity tries to make all the temperature and pressure levels coincide with the gravity levels, and in order to do that it is clear that currents of circulation are set up. In this way there is an effort to destroy the differences in temperature which have been produced by the sun's radiation and reduce them to a uniformity; that is, a uniform temperature at the same distance above the surface of the earth.

It becomes, therefore, a fundamental point in meteorology that the air over the tropics is heated in the lower levels by the action of the sun's radiation falling upon the earth, and that the air in the tropics is also lifted above its natural gravity position; hence, in the upper levels the air flows from the tropics towards the poles, and in the lower levels from the poles towards the tropics. We will not attempt to trace out this general circulation more fully until certain other conditions have been described.

If the heat is applied at the center of a canal, instead of at one end, the same principles operate, so that the lower part, being heated, has its isobars depressed in the middle, while the upper part is lifted so that the higher isobars are elevated above their original position. In this

case air flows from the center in both directions towards the cold ends of the canal in the upper levels, and from the cold ends towards the middle in the lower levels. In this figure we have therefore a general description of the primary motion of the air on the earth taken as a whole, by which the air flows from the tropics towards the north pole and the south pole of the earth, respectively, in the upper levels, and from the north pole towards the tropics and from the south pole towards the tropics in the lower levels. It should be remarked in passing that since the assumed canal is like a rectangular square box in our laboratory experiments, but as a matter of fact of a wedge shape in the earth's atmosphere, the circulation is not so easy as might be at first assumed. The meridians at the equator, which are one degree apart, converge to a point at the poles, so that the atmosphere when quiescent must be thought of as made up of a series of sectors or spherical wedges. Now the air in running from the tropics towards the poles runs from a broad end to a thin end of the wedge, and, since it can not congest, a very complex circulation is set up in order to enable it to escape unnatural compression. There are, however, many examples in the earth's atmosphere of masses of air which are arranged much more nearly in the form of a rectangular box canal, as shown when a long mass of cold air is pointing north and south, with a mass of warm air pointing from south to north and lying east of it, while another mass of cold air pointing from north to south is placed just east of the mass of warm air. While these masses may not in fact be very rectangular, yet we can study their action on the supposition that sections through them produce figures which are practically rectangular in shape. Suppose we have a warm mass lying between the cold masses, then the warm mass will be higher above and also lower below than the cold masses so far as their isobars are concerned. That is to say, at the upper surface of the sections if you want to get a mass of air at a certain density it will be necessary to go higher up in the atmosphere over the warm mass than over either of the undisturbed cold masses lying on the side, and furthermore, if one wants to get a mass of air of the same density as that lying on the under side of the cold section, it will be necessary to go down lower in the warmer mass, that is, nearer the surface of the ground, in order to find it. Applying now our principles of circulation, the action of gravity will tend to draw the upper part of the warm mass over upon the cold masses to either side, and thus tend to destroy the inequality in the elevation of the upper isobar. Similarly the cold masses will tend to flow under the warm mass from either side, and remove the discontinuity in the positions of the lower isobars. Not only do these masses of warm and cold air tend to overflow and underflow sidewise, but they seek to move, as it were, along the meridians, northward and southward at the same time; hence the long currents

of circulating air are naturally produced so that the warm and cold begin to interflow among one another, as a matter of fact in very complicated curves, the purpose of this being to restore the coincidence between the isobars, isotherms and gravity levels, which had been disturbed primarily by the heat of the sun falling upon the tropics of the earth. Having considered thus briefly the general principles which would induce circulation on a non-rotating earth, we can take up somewhat more fully the effect produced upon this same circulation by the fact of the earth's rotation; that is to say, we can discuss the circulation upon a rotating earth.

The Gyrations in the Atmosphere set up on a Rotating Earth

It will be desirable to define a few terms which occur in circulation that will enable us to speak more briefly of the subject in its advanced stages. *Rotation* will be confined to the motion of a mass of matter, as the sun or the earth, which is revolving about its center. The rotation of the earth takes place in 24 hours; the sun rotates on its axis in 27 days more or less. *Revolution* is the motion of a mass about a center from which it is separated by a radius, as the revolution of the moon about the earth, or the earth about the sun, or of an ideal particle of the atmosphere revolving about a center at a variable distance. *Gyration* is a more complicated motion. It consists of the revolution of a mass about its center at a given radius while the center itself is moving in some direction. If the moon revolves about the earth and the earth revolves about the sun, each particle of the moon will describe a series of gyrations forming a looped curve which describes this motion. If a particle of air in a tornado revolves about its axis while the axis is moving over the surface of the earth, the particle will gyrate or form a looped curve relatively to the surface of the earth. *Vortex motion* is more complex still. A vortex may be described as consisting of a series of concentric tubes. The motion of the tube is such that the inner tubes revolve about the axis faster than the outer tubes. A particle of an inner tube has a certain velocity which is greater than the velocity on an outer tube, but the velocity of the inner tube multiplied by its radius is equal to the velocity of the outer tube multiplied by its radius. If a particle moves from an outer tube to an inner tube in a vortex it can do so only by increasing its velocity of rotation. If the particle moves from the outer tube towards the inner tube and at the same time ascends along the axis, the particle will move in a helix. The *helix* may be contracting, with greater angular velocity, or expanding, with less angular velocity. In the latter case the particle moves from the inner tube to the outer tube of a vortex. A *torque* is a complicated motion which applies to a mass taken as a whole. The earth is covered by a shell of air and its actual motion may be described as a

torque. Take a bundle of paper rolled up about a center line and grasp it in the two ends. Now twist the roll so that the ends move in opposite directions. At some point in the middle there will be no motion of the particles, while the upper particles of the paper move in one direction and the lower particles move in the opposite direction. In the case of the earth's atmosphere, in each hemisphere, there are two great currents each constituting a torque. In the northern hemisphere the northern current moves eastward and is called the eastward drift. In the northern tropic the great current moves westward and is called the westward drift. There are really two torques in the earth's atmosphere, one belonging to each hemisphere, so that in the tropics, as a whole, the movement is westward, while north of the Tropic of Cancer it is eastward, and south of the Tropic of Capricorn it is also eastward. Instead of the atmosphere running from the tropics towards the poles in the meridional wedges, as a matter of fact it circulates in these great torques. In the northern hemisphere north of the latitude of 33° there is a great eastward drift and between the latitudes of 33° there is a great westward drift. Along the latitude of 33° approximately there is no general motion either east or west, and this corresponds with the part of the paper bundle which does not get twisted when the upper and lower ends are moved in opposite directions.

When a current of air moves in any direction it tends to break up into two *volutes* or spiral branches. If one takes a dandelion stem and splits it along the center, the two pieces will curl over in opposite directions and form beautiful right and left handed spirals. If a warm current of air ascends from the ground and forms a cloud, it can be seen that the middle of the cloud is ascending while the edges are descending in a gentle spiral of an umbelliform shape. If a southerly current of air moves northward it will tend to open up in two branches to the right and left. If a northerly current moves southward it will tend to open up in two branches to the right and left. The left-hand branch of the southerly current and the right-hand branch of the northerly current will tend to interlock or intercurl. The great current in the tropics which moves westward, instead of proceeding due west around the earth, tends to break up into two great volutes, one curling into the northern hemisphere, and one curling into the southern hemisphere. The word *curl* has several meanings, the first is that in which it has already been used; namely, a spiral rolling about a center. The second is connected with vortex motion and is really a name for a part of the helix action. If a mass of air moves in a spiral towards a center, it is evident that it can not proceed long in this way without some provision for its escape. If it moves in a spiral on a given plane it must begin to escape along a line perpendicular to that plane. If a circle is taken as a boundary in a given plane, and a certain mass of

air moves into this circle on spirals, then there will be a certain amount of the air moving perpendicular to the plane of the circle. This whole action of spiral movement inward and vertical motion from the plane is called a curl and it depends upon vortex laws. In a tornado or hurricane the curl is illustrated by air which ascends as a current while the air is moved inward along certain spirals. It is also illustrated in electricity and magnetism where an electric current passing around the helix surrounds a magnetic field perpendicular to the sections of the tube along which the electric current is flowing. Electric currents and magnetic fields are also related to each other by the law of the curl, and this evidently goes back to the idea of the helix or vertical spiral.

We may now resume our discussion of the circulation of the air on the rotating earth, repeating to some extent what has been said in defining these special terms. Take a globe and in the tropics place an arrow pointing westward between the equator and the latitude of 33° both north and south. To the north of 33° place an arrow pointing eastward, and in the southern hemisphere to the south of 33° place an arrow also pointing eastward. These represent in a general way the action of the atmosphere as consisting of two great whirls in each hemisphere, thus composing a torque on a hemispherical scale. Draw a ring around the earth in latitude 33°, cutting out a section of the atmosphere. If this ring moves northward it will evidently contract, and to have the same angular momentum, that is, mass energy, it must rotate faster about the axis as it approaches the pole. This constitutes in a way an illustration of vortex action whereby a particle passes from an outer to an inner tube and consequently revolves faster about the axis. Take another section south of latitude 33°, cutting out a ring of atmosphere. If this ring moves southward it must rotate slower because it is moving to a region at greater distance from the earth's axis if it is to retain the same momentum or energy of mass in motion. The importance of these great torques in the earth's atmosphere can be seen from this general fact that while the weight of the earth's atmosphere taken as a whole is very great, and is, generally speaking, in vigorous motion, yet the currents as a whole are so interbalanced that the mass energy moving eastward is exactly equal to the mass energy moving westward when the whole atmosphere is summed up. This is proved by the fact that the rotation of the earth on its axis does not change by the smallest fraction of a second from century to century, or at least astronomers have been unable to detect any change in the period of the earth's rotation so long as observations have been continued. If this balance of eastward and westward momentum were not perfect, it would immediately be shown by a change in the period of the rotation of the earth upon its axis.

The picture which is presented by these ideal rings starting from latitude 33° in each hemisphere and moving respectively towards the poles and towards the equator is not very complete, because the rings do not continue to move as a whole with an increase or decrease of velocity. If we examine the actual velocity of the air in a given latitude, as over the city of Washington or again in the tropics, as over the Barbadoes, we shall find the following facts: At the ground in Washington the wind averages about six meters per second eastward; at an elevation of 2,000 meters the eastward velocity is about eighteen meters per second; at 4,000, it is about twenty-four meters per second; at 6,000, it is about twenty-eight meters per second; and at 10,000, it is about thirty-four meters per second.

Above this level the air moves eastward at a rate of about forty meters per second; that is, ninety miles per hour. That is to say the eastward velocity or the eastward drift increases upwards with the distance from the ground. Now these velocities are maintained throughout the year with certain seasonal variations, though, of course, they are at times disturbed by certain local circulations as when storms disturb the normal movements of the air. The gyrating rings then which we first considered may be more accurately described as sheets of air parallel with the earth's surface which flow over each other at different speeds, the upper sheets flowing faster than the lower sheets. This may be practically seen in the cirrus clouds which are higher than the cumulus clouds, and move eastward as a whole with twice as great velocity. It is evident that we have here another type of vortex motion. What we first considered in the course of our definition was a vortex in which the inner rings rotate faster than the outer rings, but in this case of the torque in the northern hemisphere, for example, we have the upper rings moving faster than the lower rings. This apparent inconsistency may be reconciled by assuming that the axis of the upper rings instead of being a line, as in the other case, is really a spherical surface high above the ground outside the earth's surface, to which the actual motion has to be referred. Mathematically considered such a spherical sheet is in certain aspects equivalent to a line so far as the reference of motion is concerned; that is, the motion may be a maximum along a spherical sheet in one case, or a maximum around an axial line in the other case.

Turning now to the tropics and examining the motion of the air in a vertical section just as we did in the north temperate zone, we find that the westward motion is distributed very differently. At the surface the westward motion is greatest, and it decreases gradually on going upwards from the ground till at 10,000 meters or so it has decreased to zero, and above that region an eastward motion sets in, gradually increasing with the height. The westward branch of the torque

then, is strongest at the surface and decreases upwards. The eastward branch of the torque is a minimum at the surface and increases upwards. We have several times referred to the latitude of 33° north and south of the equator as separating the eastward branch from the westward branch of the torque, but it has now been indicated that at about 10,000 meters above the tropics the westward branch changes into an eastward branch of the torque. As a matter of fact the surface which separates the westward branch from the eastward branch spans the tropics in an arch resting on the ground at 33° of latitude and crossing the equator at 10,000 or 12,000 meters above it. Beneath this arch the western torque is included with its maximum motion at the bottom; above this arch with a broad base in each temperate zone rises the eastward torque in which the velocity increases upward and gradually overspreads the tropics in the higher elevations, the northern branch reaching southward, and the southern branch reaching northward in a comparatively thin shell till they touch somewhere above the equator. All this circulation therefore constitutes a complex vortex which can be referred to distinct mathematical laws. If the atmosphere were willing to circulate in this simple manner it would not be difficult to adapt our mathematical analysis to it, but unfortunately, instead of moving so that the branches of this torque remain intact and retain their theoretical individuality, there is a continual interchange or passage of currents from one branch to the other in a rather irregular way which it will be necessary more closely to examine.

The Circulation on the Rough Rotating Earth

The circulation which we have been describing might possibly be set up on a perfectly smooth globe having the size and shape of the earth, but the presence of continents and ocean areas, the mountain ranges stretching north and south on the American and east and west on the Euro-Asiatic continent, facilitate the breaking of these theoretical branches of the torque into great circulating masses which interplay among each other. It is evident that the Rocky Mountains of North America and the Cordilleras of South America tend to stop the westward currents in the tropics and the eastward currents in the temperate zones. On the other hand, the Himalaya range in Asia tends to hold the westward current in the tropical zone and the eastward current in the temperate zone. There are thus certain places, that is, certain longitudes, where the currents tend to curl from the tropics into the temperate zones. A conspicuous instance of this occurs in the United States, where there is a continual outpouring of warm air from the Gulf of Mexico over the Mississippi and central valleys of the United States. While the trade winds in the tropics tend to blow from the northeast, it is known that immense masses of air move

from the tropics over the United States from the south, quite contrary to the general principle; and similarly, though not so conspicuously, a case is found in South America and south Africa. On the contrary, the warm air in the lower levels over the Indian Ocean, whose winds are called monsoons, simply beats upon the great mountain ranges to the northward of India without penetrating the temperate zone in Siberia. In this way certain great circulations called centers of action form in each hemisphere. There is one over the middle Atlantic Ocean; another over the middle Pacific Ocean of the northern hemisphere; and there are other corresponding centers of action in the southern hemisphere. These are especially well marked during the summer time when the ocean is cool and the land air is warm. In the winter time the tendency is to form centers of action over the land areas instead of over the ocean areas, but the process is much more irregular, and in the United States it is exhibited chiefly by a succession of cold waves which traverse the United States from west to east. Referring to the center of action over the middle Atlantic Ocean in summer, we know that the winds near the American side are from the south or southwest. On the Atlantic Ocean in latitude 35° to 40° north the winds are blowing eastward, and in latitudes 25° to 30° they are blowing westward; on the European side they are blowing from the northwest and north. The consequence is that the United States is bathed during the summer with warm, moist winds in the eastern half, and with warm, dry winds in the western half of the continent. In Europe, on the contrary, the northerly winds prevail, and it follows that the American continent is warm during the summer while Europe is cool, and this is the cause of the annual migration of tourists from America to Europe instead of from Europe to America. The control of the climate of Europe by the American Gulf Stream is a myth. As a matter of fact the European climate is controlled by the great currents of circulation referred to these centers of action.

More generally, warm masses of air find their way from the tropics into the temperate zones by very irregular paths, and cold masses find their way from the northern latitudes into the temperate zones by very irregular paths. A similar statement applies to the circulations of the southern hemisphere. The disturbances in the general circulation which are produced by the land and ocean areas make it well-nigh impossible to reduce meteorology to any simple scientific system. The irregularities produced by the interaction of these warm and cold masses are so great that the problem of forecasting seems to bid defiance to any clear classification. The eastward drift over the United States is, of course, the basis of any possible forecasts, and the irregularities caused by the interpenetration of the warm and cold masses, one after the other under the action of gravitation, produce

what we call our storms, but technically are called cyclones and anticyclones. It would be beyond my province to attempt to analyze the technical side of the theories of cyclones and anticyclones, and yet the subject of circulation would be so incomplete without at least alluding to the prominent attempts which have been made to solve these great questions that I shall venture a few remarks along this line.

The circulation of the air is classified as general and local, "general" applying to the whole hemisphere, of which some description has been given, and "local" as applying to the individual storms and their accompaniments. The local storms are known as cyclones and anticyclones, hurricanes, tornadoes and water spouts. They are all features or phases of the circulation and can be referred back to a few simple mathematical laws. Two attempts were made to solve the question of the general circulation, but the year 1896–7, which represents a new era in meteorology, when the international cloud observations were established under the leadership of Dr. Hildebrandsson, marks an epoch in the theory of the subject. I refer to those of Ferrel and Oberbeck regarding the general circulation. They had one picture in mind, namely, that of the simple canal, heated at one end, to which allusion was made in the early paragraphs of this lecture. They conceived the air to flow from the tropics northward towards the poles in the upper levels, and from the poles towards the equator in the lower levels, the northward current being separated from the southward current by a neutral plane along which there was no motion. Ferrel discussed the equations of motion adapted to the general hemisphere, and threw considerable light upon the subject. He conceived the rings of parallel 33° to move towards the poles with increasing velocity, and he made serious efforts to account for the fact that the velocity in higher latitudes is comparatively moderate instead of excessively great, as his equations demanded. If Ferrel derived an excessive velocity near the poles, Oberbeck, as a result of his complex integration, derived an excessive velocity in the upper levels over the tropics. Neither of these authors accounted for the reversal of direction from west to east at a moderate elevation, as 10,000 meters over the tropics, nor did they attempt to take into account the great irregularities in the circulation in an east and west direction, which we have described in discussing the centers of action. The result of the work of the Weather Bureau in the international cloud observations in the year 1896–7, was to destroy this theory of a neutral plane separating the upper northward current from the lower southward current. In place of that it was explained that these interchanging currents, instead of passing over each other at different levels, really interpenetrate and pass by each other on the same level; that is to say, warm air moves from the tropics towards the poles in all levels, and cold air from the poles towards the tropics in

all levels. The first theory can be illustrated by sliding the fingers of the smooth hands over each other in opposite directions, while the second theory can be illustrated by sliding the fingers between one another on the same level; the fingers of the one hand will represent the warm currents and the fingers of the other hand the cold currents. This new view is really revolutionary because it renders inapplicable the integrations which were attempted by previous authors. Unfortunately the problem has become in this way so very complicated, that no one of sufficient ability has yet been found to carry out the necessary mathematical analysis with anything like fullness or precision. At present meteorologists are engaged, by means of balloon and kite ascensions, in determining the nature of the currents from the south and from the north which prevail in different localities. Europe has already done a great deal of work in this direction, and the United States has recently made a beginning. A few soundings have also been made over the Atlantic Ocean. Generally speaking, however, this is a great field of research which it will require much money and time to adequately complete. The circulation of the atmosphere, therefore, is a great and fascinating problem for future development, and indeed it may require more than one generation of scientists to bring it into subjection.

We have described the cold and warm currents as interpenetrating on the same levels like the fingers of the two opposite hands. Gravitation takes these warm and cold masses and seeks to make them interpenetrate yet more intimately, so that the warm masses will become more cooled, and the cold masses more warmed, and the isobars and isotherms coincide with each other and the gravity levels. It is a curious fact that masses of warm and cold air having any size are exceedingly reluctant to mix with one another; that is to say, the interchange of heat is a molecular process which naturally goes on slowly, and in accomplishing it, in the atmosphere, a great deal of energy must be expended. The great masses are first torn into shreds along their edges, and are gradually fritted away in the local cyclonic circulation. The energy that is felt in storms of any kind is merely an illustration of this thermodynamic process of interchanging temperature.

The Local Circulations

Historically speaking, the year 1896–7 marks the beginning of a period of transition in the history of local as well as general theoretical meteorology. There have been two schools of meteorology: one American, whose head is Ferrel, and one German, of which Guldberg and Mohn, Sprung, Oberbeck, Margules and Pockels are leaders. These two schools agreed in one particular, namely, in that they assumed that the cyclonic and anticyclonic circulations are symmetrical about a center.

The first break in this theory was also made by Professor Bigelow in the cloud work of the international year, when it was shown that the distribution of warm and cold masses in the anticyclone was not symmetrical but asymmetrical. In the symmetrical theory the center of motion coincides with the center of heat or center of cold; in the asymmetrical theory the center of motion is located near the edge of the warm and cold masses. The actual cyclone is warm on the one side and cold on the other side of the center, and likewise the anticyclone is cold on one side of it and warm on the other side of it. The northerly cold current, therefore, has a cyclonic center on the east side of it and an anticyclonic center on the west side of it, while the southerly warm current has an anticyclonic center on the east side of it and a cyclonic center on the west side of it. These differences are also fundamental. Ferrel treated the equation of motion by one solution, quite similar to that which he applied to the general circulation of the hemisphere, and he found the vortical torque for the cyclone clockwise on the outer part, anticlockwise on the inner part, with complex lines of flow connecting them. The theoretical difficulties are quite obvious when we consider that such a vortex as Ferrel worked out is applicable only to a fixed mass of air; for example, put a mass of water in a cylindrical vessel and sprinkle sawdust in it so that the stream lines can be followed by the eye. If now heat is applied to the center it will boil along the stream lines indicated by Ferrel's vortex, and especially so if the glass vessel is rotating on its axis. This would make our cyclones storms in which the same mass of air is boiling over and over again along these fixed lines, whereas we have shown that the cyclonic circulation is simply built up by currents of air which are streaming through it in a very irregular way, and, anticipating the conclusion which we have reached in our research, it may be asserted that the cyclone, besides being asymmetrical, conforms only loosely to any known type of theoretical vortex. The German school of meteorologists also discussed the symmetrical vortex, but by another mathematical process. There are two other solutions of the second equation of motion, one of which was assumed to apply to the outer part and the other to the inner part of a cyclone. The solution for the outer part has no vertical current, while the circulation for the inner has a vertical current, quite like that in the vortical helix, such as may be illustrated by the ordinary tornado tube. Many attempts were made to join the outer part and inner part in a single set of equations, the results conforming very loosely to the observed facts in nature regarding the velocity and angular directions. It is not too much to say that neither of these systems of solution will find more than a very small application in practical meteorology. Ferrel discussed the three equations of motion, one by one, giving certain practical inferences which he found more

or less illustrated in nature, but he never succeeded in uniting the three equations in one comprehensive system. The Germans approached more nearly a satisfactory solution, but as already stated, the assumption that cyclones and anticyclones are symmetrical, respectively, about warm and cold centers, is no longer tenable. We have already made the assertion that the asymmetrical cyclone, as it occurs in nature, does not conform satisfactorily to any homogeneous vortex. It will be possible to show how this is by giving a few details regarding waterspouts, tornadoes and hurricanes, which will lead up to this conclusion.

Local Vortices in the Earth's Atmosphere

A large waterspout was seen at Cottage City, Mass., in the Vineyard Sound, on August 19, 1896, about eight miles distant from Cottage City. Fortunately a series of good photographs was secured of the waterspout and its cloud, which together with the meteorological data, have enabled us to compute the dimensions and the velocities of motion in all parts of it by means of the vortex formulas. It happens that the same cloud developed two types of vortex, at short intervals of time between them. One is the funnel-shaped vortex and the other is the dumbbell-shaped vortex. Fig. 1 gives an illustration of a section

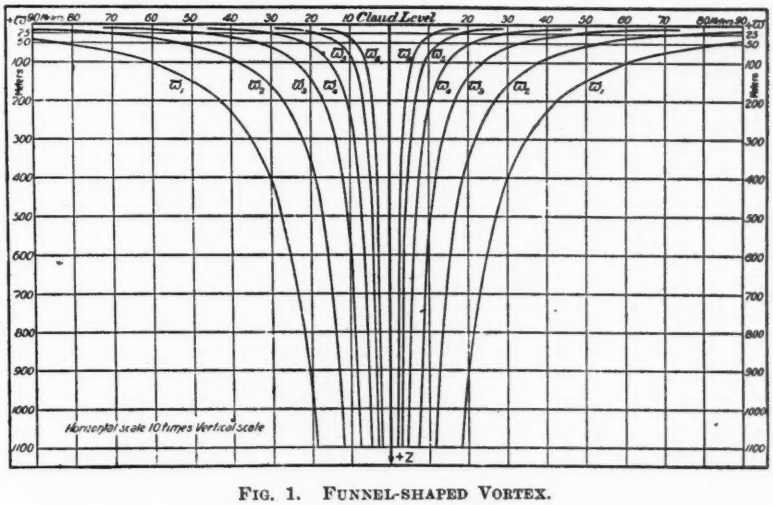


Fig. 1. Funnel-shaped Vortex.

through the funnel-shaped vortex, and shows the boundary of the several vortex tubes. The horizontal dimensions are multiplied by ten for the sake of showing the relative dimensions more plainly which exist from one tube to another. It will be noted that the distances between the successive tubes get smaller and smaller in a geometric ratio towards the axis. They concentrate at the lower part, and expand so that the lines become parallel to a horizontal plane in a region at

the level corresponding to the base of the cloud. Fixing attention upon any one of these tubes, as the first or outer one, the theory indicates that a particle of air which is lying on that tube in the lowest level continues throughout its motion to follow the same tube. This particle rotates in a spiral about a central axis gradually rising from the ocean towards the cloud, and, rotating in larger and larger spirals, at last it flows out from the axis parallel to the surface of the cloud itself. On this outer tube the particle at the sea is moved with a velocity of 22 meters per second and gradually rises upwards and changes its velocity through 20, 18, 15, 12, 7, 5, 2 meters per second quite near the surface of the cloud, and finally the velocity falls to zero. At the sea level the velocity is in a circular direction around the axis; at the cloud level it is moving in a radial direction directly away from the axis. On the outer tube having a large radius the velocities as already given are small, but on the same levels on tube No. 5 quite near the axis the velocities on the same levels become, respectively, 182, 159, 136, 110, 80, 44, 31, 14 meters per second near the cloud level, and they finally run out to zero. With such enormous velocities as 182 meters per second at the sea level, the causes of the turmoil and destructive effects which are always found in the case of waterspouts and tornadoes passing over the land are readily appreciated. Illustrations of the destructive effects of tornadoes are commonly accessible. The purpose of such a vortex is to lift a mass of air, as in a suction pump, from the surface of the ocean to the cloud, and in this case it is computed that 2,468 cubic meters of air are lifted in each second through each section of this vortex tube. These natural lifting pumps are evidently of great efficiency.

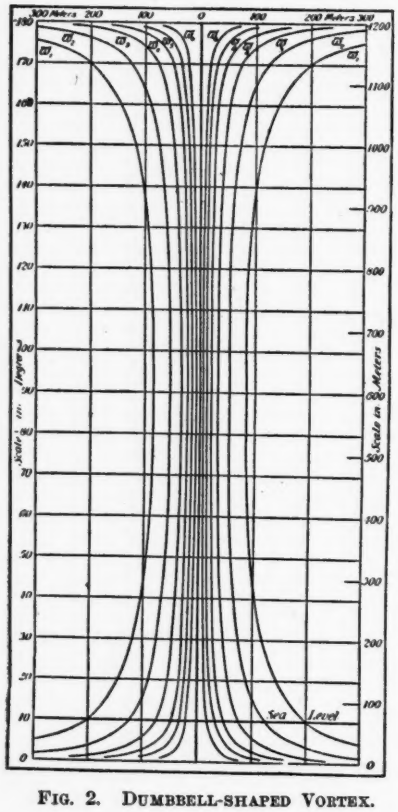


FIG. 2. DUMBBELL-SHAPED VORTEX.

The dumbbell-shaped vortex operates on substantially the same principles, though the details are different. In this vortex the air begins at the sea level to flow inwards towards the axis in a spiral which contracts up to about 500 or 600 meters above the surface of the sea, and then it begins to expand as in the funnel-shaped vortex. The dumbbell-

shaped vortex is really composed of two funnel-shaped vortices, the lower one pointing upward and the upper one pointing downward, meeting half way between the two planes of reference. This vortex is really a more efficient lifting pump than the other one just described, and it is found that 16,452 cubic meters of air are moved upwards through each tube per second, so that the dumbbell-shaped vortex is carrying 6.7 times as much air upward as the funnel-shaped vortex. A careful examination of this dumbbell-shaped vortex at Cottage City shows that the lowest sections are not fully developed. The outward curvature of the tube is plainly shown on the picture, but at sea level it is cut off or truncated by the friction of the tube against the water of the ocean. The cutting off of these vortices at some section above their theoretical lowest plane seems to play an important part in practical meteorology.

On May 27, 1896, a violent tornado of large dimensions passed over the city of St. Louis, causing great destruction in Lafayette Park and thence to the bridge over the Mississippi River. The enormous power of the forces which accompanied this vortex is shown on many pictures which were secured at that time. Large trees were twisted off

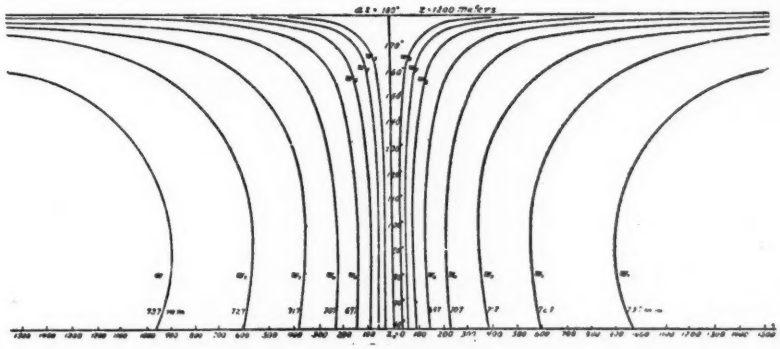


FIG. 3. TRUNCATED DUMBBELL-SHAPED VORTEX.

and stripped of their branches; buildings were overturned and destroyed in every conceivable way; heavy iron girders and stone work of the bridge were destroyed; and in short almost limitless powers seem to have been at the disposal of this great vortex. Fig. 3 shows a section of this vortex, the relative distance apart of the tubes, and the part which has been cut off or truncated at about one third of the distance from its lower plane of reference, several hundred meters below the surface of the ground. It has been shown that this St. Louis tornado was about 47 times as efficient as the large Cottage City waterspout in its lifting power, and that at the surface of the ground it developed somewhere between 150 and 250 meters per second; that is, 340 to 560 miles per hour. While it is not probable that these enormous theoret-

ical velocities can develop near the center of a great tornado on account of the retarding effects of friction where the wind moves over a rough region like a city, yet it does show where the enormous power resides that is always observed in these conditions. It might develop, therefore, a pressure of 5,000 or 6,000 pounds per square foot. This is, of course, very much more than would be necessary to make all the destruction that has been noted.

Hurricanes such as are observed in the neighborhood of the West Indies, and the typhoon, which is the name of a hurricane in the neighborhood of the Philippine Islands and China Sea, are truncated dumbbell-shaped vortices built on exactly the same principles as the St. Louis tornado, only they are very much larger in their dimensions.

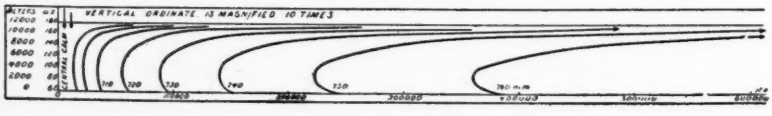


FIG. 4. HALF SECTION OF A HURRICANE VORTEX.

The tornado generally ends at a level something like 1,200 meters above the ground, and it is usually much less than half a mile in diameter. The hurricane, however, is probably 12,000 meters thick, and it extends several hundred miles in diameter. This makes the hurricane a very thin mass of air of broad extent, as compared with the word tornado, which is a relatively high mass of air and narrow in extent. We can construct the velocities in the hurricane from our meteorological data, and show that the winds blow at a certain angle, which conforms to the section that cuts off or truncates the vortex at a certain plane. These angles should be more fully explained. On the lowest plane the wind flows radially and directly towards the axis; on the uppermost plane it flows radially and directly away from the axis; at a middle section, half way between these two planes, it flows in circles tangentially around the axis. In passing from the lower plane to the upper plane the wind gradually makes a larger angle with the radius; first 10°, then 20°, then 30°, and so on up to 90° at the middle plane half way up the tube; then 100°, 110°, and so on up to 180°, which represents the wind flowing radially away from the axis. If now a vortex is truncated at a certain plane, all the winds on that plane will make a given angle with the radius. If a truncated plane is one third the distance up the axis then the wind will make an angle of 60° with the radius; that is, it will blow in at an angle of 30° from a circle. This is about the angle of the wind which observers have recorded in the case of hurricanes, and hence it is proper to infer that the truncated section should be drawn at about the distance indicated from the lower plane.

The *ocean cyclone* is a mass of air still larger than the hurricane,

circulating on practically the same vortical laws, but unfortunately it shows indications of not being able to follow the law strictly, especially in the inner portions of it. The outer part of a strong ocean cyclone, where the barometer drops to 28 inches of pressure at the center, is very much like an enormous hurricane in its formation, but near the center the angles and the velocities begin to break away from the pure vortex law. This is probably due to the great extent of the wind areas, and consequently the congestion, and to the fact that the ocean cyclone is not deep enough, although it may be 3 or 4 miles high, to carry out fully the requirements of so large a vortex of a pure type. It is known that hurricanes are vortices which are 6 or 7 miles deep. The large ocean cyclone is probably not more than 4 miles deep, and the great land cyclone is rarely more than 2 or 3 miles deep.

The *land cyclones* in the United States conform to the pure vortex law less perfectly than does the ocean cyclone. The pressure in the land cyclone usually stops at about 29 inches near the center. Its depth is usually about 2 or 3 miles. It may cover a diameter of 2,000 miles. These dimensions are evidently unfavorable for the development of a pure vortex. Furthermore, the distribution of the temperature in the land cyclone is entirely different from that in the pure hurricane, and this too prevents the land cyclone from developing according to the perfect law. Furthermore, the cyclones of the temperate zone develop in the lower levels of the great eastward drift. In these lower levels the eastward velocity of the drift is not very high; something like ten meters per second. At the height of two or three miles the eastward drift is something like twenty to forty meters per second. It becomes evident, then, that a vortex which develops in the lower levels, from any set of causes, must lift its head into a rapidly flowing stream of air, and this necessarily will tend to break down the intruding head by stripping off portions of it and detaching the upper portions of the vortex from the lower portions. Now a vortex can not develop except as a complete individual. If it is intruded upon by cutting off the lower section, as in the hurricane over the ocean, or by the upper sections thrusting themselves into the stream of the rapidly flowing eastward drift, it is evident that this is a sufficient cause for the partial destruction of the vortex system. In the theoretical vortex, above the middle section, the wind has an outward component increasing with the height, as already explained. Below this section it has in every cyclone an inward component. Now as a result of the cloud observations which were undertaken by the U. S. Weather Bureau during the international cloud year 1896–7, in which between 6,000 and 7,000 observations were made by means of theodolites upon the direction of motion in the different cloud levels, it was found that there was an inward component over the cyclones in all levels from the ground up

to four or five miles high. The strongest inward component was in the strata cumulus levels about two miles above the ground. Above this level and below it there was a radial inward velocity of a certain value corresponding to each level. There was no clear indication that the inward component in the lower levels reversed to an outward component in the upper levels, and it looked as if the intruding vortex of the lower levels did not succeed in reaching the middle plane where theoretically the outward component begins to develop. It looked as if this vortical system was so stripped of its natural features, by the action of its intrusion into the eastward drift, that only the lower half of the vortex really survived, and that there was an insistent struggle of the rotating cyclone with this eastward drift for the mastery. In a word, the upper sections of the vortex were stripped bare, and they gradually died out at the height of three or four miles within the eastward drift. What remains then is a set of stream lines in the lower levels which have certain features in harmony with the pure vortex system, though only roughly conforming to them, and which in the upper levels is broken down into a very imperfect kind of vortex. It should be said in passing that it is very difficult, on account of the prevailing clouds which occur in the cyclones of the United States, to get satisfactory measures of the cloud motions in the upper levels. Cumulus clouds develop strongly below the one-mile level, and above them it is possible to get the cloud motions in the higher levels only through the more or less occasional rifts in the lower cloud sheets. It is therefore very desirable that an extensive campaign of theodolite measurements of cloud motions in the upper levels of cyclones be instituted, in order to carry out much more fully the details of the discussion which have been suggested in this fundamental research.

Temperature Distribution

Having now described the general and local circulations in the temperate and tropical zones, it is important to make some further remarks regarding the distribution of temperature in those regions, also including the distribution of temperature in the sun itself. The circulations which take place are accompanied by certain changes of temperature in a vertical direction, called temperature gradients, which are characteristic of them. If a cubic centimeter of air at the sea level is lifted up to higher levels, so that it cools simply by the expansion of its own mass, and there is no mixture of warmer or colder air with it from the outside, then the temperature will fall 9.87° Centigrade for every 1,000 meters. Now it is found by balloon observations that the temperature gradients in different regions do not conform to this fundamental rule, which is called the law of adiabatic expansion. In the tropics in the lower levels this rate is very nearly approached, but there

is a considerable deviation from it in the upper levels. In the temperate zones the normal vertical temperature gradient is only about 5.40° Centigrade, though it may be considerably more or considerably less according to the circumstances. It may be generally said that, except in restricted regions, the air does not cool as fast in going upwards as it should if it were caused by mere vertical expansion. The upper levels of the air are too warm; warmer than they should be if that law prevailed. In the temperate zones they are very much too warm, and that is why the vertical gradient is less than it should be according to that law. The fact is that the warm masses of air which flow from the tropics towards the poles retain their heat above what they should have for the given latitude, and in that way the upper levels of the atmosphere are maintained at a considerably higher heat than would be expected. When the air has once cooled to about 70° below zero, Centigrade, it seems disinclined to cool much further, and in the levels from 12,000 to 16,000 meters high there has been discovered a tendency for the air to be somewhat warmer than it is in the levels below, say from 8,000 to 12,000 meters high. It is generally thought that this phenomenon is due to radiation in some of its effects, but it is still a subject of discussion. If we should assume as the average vertical gradient for the entire atmosphere a rate of about 7° Centigrade per 1,000 meters then we should find that the temperatures in the tropics fall off too fast, and in the temperate zones too slow to conform to this average gradient. Now the mathematical law shows that if the lower levels of the atmosphere are relatively too warm for the upper levels there will be a westward drift as in the tropics, and if the upper levels are too warm relatively for the lower levels there will be an eastward drift as in the temperate zones. Speaking a little more broadly still, in order to avoid discontinuity, that is to say, changes by jumps in the atmosphere as regards the barometric pressure at the different levels, since the warm air has less density than the cold air, it follows that the warm air must move faster over the surface of the earth than does the cold air. Hence it is that in the tropics the air is too warm for its altitude, and it must move off faster than it otherwise would in the tropics. The westward drift in the lower levels compensates for this excessive temperature, and in the upper levels of the temperate zones the excess of motion compensates for the higher temperature. We find exactly the same principle working in the formation of hurricanes and tornadoes. Hurricanes develop in the northern hemisphere in the late summer and early autumn, and this is the season when the cool air of the northern latitudes begins to spread southward towards the equator as the sun begins its southward march into the southern hemisphere. At first the cool air flows over the warm air in the higher levels. This in a general way increases the vertical tem-

perature gradient, and induces a more lively movement in the lower levels. In certain localities, in order to keep up the vertical continuity of barometric pressure, the warm air slides out radially in all directions, where conditions are right, and this movement first induces the vortical action in the upper sections of the hurricane which is gradually propagated, when it is highly developed, to the surface. Tornadoes are formed in somewhat a similar way, but in this case the cold and warm masses lie side by side on the same level, though there is a tendency for the cold air to overflow the warm air. The sliding action of the warm air against the cold sheet is the first incentive to the curling-up process which culminates in a tornado. In the ordinary cyclones the temperature distribution is such that the vertical gradient is about the same in the cold as in the warm mass, taken separately, though there are moderate variations in the different quadrants surrounding the high and the low areas of pressure. The warm air then overflows the cold air in two branches, and the cold air underflows the warm air in two branches. This tends to induce vortical action, but as already explained it is retarded, and the development is very imperfect on account of its intrusion into the eastward drift.

While our knowledge of the distribution of velocity and temperature in the atmosphere of the sun is much less perfect than it is of the atmosphere of the earth, we have yet definite knowledge regarding several important features. Apparently the sun's atmosphere does not operate in the same way that we have found to be the method of the circulation of the atmosphere of the earth. It is quite easy to see that these two atmospheres should work in a very different way. The atmosphere of the earth is really a thin shell of air heated in one zone by the solar radiation falling upon it, and then this thin shell simply slides around over the surface of the earth according to the laws which have been described. In the case of the atmosphere of the sun we have no definite knowledge as to its depth. It is proper to infer, from the law of pressure and mass, that the density near the center is such that the interior of the solar mass consists of a nucleus in a highly viscous or even solid state. Such a nucleus may be only one third of the diameter of the sun, but as the radius of the sun is 694,800 kilometers it would make a nucleus of about 400,000 or 500,000 kilometers in diameter. Above this the shell of the sun would be something like 400,000 kilometers thick, that is, about 250,000 miles. Our observations can not penetrate below the surface of the solar photosphere, and of course it is impossible to trace out the great currents which are undoubtedly operating within this enormously thick mass. On the surface we know from various sources that at the equator the solar mass drifts from east to west as we look at the sun's disc with a velocity such that the sun turns on its axis, as we see it, once in 26.68 days. This rota-

tion of velocity falls off gradually towards the poles, until at the poles it takes something like 30 days to turn around. There is evidence to show that in the polar zones or near the poles there are certain variable velocities of rotation. These may belong to different sections in the sun's atmosphere. Our observations at the poles cut through the sun's atmosphere, as it were, parallel to the surface. At the equator our observations look down vertically through the sun's atmosphere. We can therefore near the poles get the same kind of observations at different solar levels. However this may be, the turbulence of motion seems to be much greater near the poles than near the equator. Within the sun's mass we can well imagine that many different periods of rotation, or of the daily angular velocity, actually exist. Looking at the solar surface as a unit, it consists of a huge wave whose crest advances around the equatorial regions at a considerably greater speed than in the polar regions. Now our mathematical analysis indicates that such a circulation can be maintained if the solar temperatures are greater in the polar regions than in the equatorial regions. That is a form of vortex, applicable to the solar mass, in which the velocities and temperatures are so connected together that the polar regions are warmer and have a slower angular velocity than the equatorial regions which are cooler with a greater angular velocity. This, therefore, is a condition of affairs practically the inverse of what we have been describing in the atmosphere of the earth. It is of course in some way associated with the great heat cauldron which is boiling inside the solar surface, where the heat accumulates and congests and finally works its way to the surface by means of this gigantic solar vortex. Within the great vortex there are innumerable minor vortices. These vortical tubes generally stretch from north to south perpendicular to the plane of the equator. These vortex tubes may be very irregular and broken up, but as a whole the sun may be described as a polarized mass throughout which the minor motions are nearly parallel to the plane of the equator.

Solar Phenomena

The different levels in the sun's atmosphere have received the following names: The lowest one which is visible is called the *photosphere,* and consists of mottled shapes like cumulus cloud forms, bright and dark areas being interspersed. Above this is the *chromosphere,* a layer of hydrogen and calcium and other gases 5,000 or 6,000 miles thick. The lower surface of the chromosphere is a reversing layer, so called, and is the level at which the dark lines of the solar spectrum are formed. Through these layers are projected jets of hydrogen and calcium flames which stretch out beyond the visible edge of the sun called *prominences,* and far beyond the region of the prominences

extends the *solar corona* which reaches enormous distances into space. The corona is apparently composed of minute dust particles and ionized atoms and molecules held in certain positions by the action of electric and magnetic forces. The photosphere is penetrated in certain regions by *solar spots* which extend from the upper levels of the photosphere into the interior. It has been shown by recent photographs taken at the Mount Wilson Observatory that the sun spots are closely associated with motions so like those pertaining to the sections in a dumbbell-shaped vortex that the analogy appears to be very complete. If this is so, then terrestrial meteorology becomes intimately connected with solar meteorology in many of its features, in spite of the great differences of temperature. The average temperature of the earth's atmosphere may be taken as about 15° Centigrade below zero. The surface of the photosphere is apparently between 7,000° and 8,000° Centigrade, and the sun's temperature increases to more than 10,000° near the nucleus, though the gradient is not yet known. Taking the sun spot region as a whole, the sun spot belts form near latitude 30° north and south of the equator and they gradually drift towards the equator in the course of about eleven years, when new spot belts begin to form. The same is true of the faculæ which are closely associated with sun spots. The circulation within the sun spot belt is from the surface downwards, while the spots drift as a whole towards the equator. This indicates, therefore, descent into the sun from the surface in the neighborhood of the equatorial regions, and, of course, to compensate this, material must be projected from the interior outwards in the higher latitudes.

The *prominences* are hydrogen flames going through a periodic drift. They may be said to appear first in largest numbers in middle latitudes, and they seem to divide into two branches so far as the number of them is concerned. One branch drifts southward in the eleven-year period along with the spots and the faculæ. The other branch drifts poleward to the north and to the south, respectively. A study of the number of these prominences in different latitudes indicates that there is a periodic change in the apparent velocity of the rotation in the polar regions, fluctuating back and forth in about a mean value. Since the prominences have different elevations, and different levels in the sun have different velocities, it may well be that in the polar regions the prominences develop sometimes in the higher levels and sometimes in the lower levels, so that they actually drift eastward at different angular velocities according to their elevation. The spectroscope apparently indicates a certain angular velocity pertaining to special spectrum lines, which look like a fixed value for a given elevation, and at the same time it has been shown that hydrogen

lines have different values from iron lines, and therefore the entire subject is open to an extensive investigation.

The atmosphere of the earth is filled with what is called *atmospheric electricity.* This consists of positive and negative charges of electricity distributed in a very complex way, depending upon temperature, vapor contents and barometric pressure. The distribution of electricity changes with the season of the year, and with the hour of the day, and differs from latitude to latitude, and from elevation to elevation above the same place. Similarly the sun's atmosphere is filled with electric charges. Every electric charge in motion produces magnetic field. If particles of electricity rotate about an axis, and parallel to a given plane, there will be a magnetic field perpendicular to that plane. These magnetic fields may occur at any temperature, provided the charge of electricity and the motion in a closed curve is at hand. If a ray of light in a strong magnetic field is looked at along the lines of force of the field, a single ray is split up into two lines slightly displaced and circularly polarized in opposite directions. If the line of light in the magnetic field is looked at perpendicular to the lines of the magnetic force, a single is split up into three or more lines. In the case of three lines the outside lines are displaced and polarized in one direction, while the middle line is not displaced but is polarized at right angles to the two side lines. These effects of the magnetic field upon a ray of light are called the *Zeeman effect,* and if these effects are seen it is strong evidence if not proof that the magnetic field has been acting upon the ray of light. The Mount Wilson Observatory has been able to show that the light which comes from the interior of the sun spot or vortex produces both types of the Zeeman effect, the two circularly polarized lines when one looks at the spot near the center of the solar disc; that is down the tube of the vortex; and the three plane polarized lines when one looks at the sun spot near the edge of the sun, that is, nearly at right angles to the sun spot vortex tube. At any rate enough has been shown to make it more than probable that magnetic field exists certainly in the solar spots, and probably throughout the mass of the sun where gyrations and internal vortices doubtless take place. If the sun spot produces a magnetic field strong enough to show the Zeeman effect at a distance of 93,000,000 miles, it is entirely reasonable to suppose that magnetic fields occur through the solar mass wherever there is actual circulation. It has already been intimated that the entire body of the sun consists of an enormous number of circulating tubes directed more or less perpendicular to the equator, and as a corollary the entire mass of the sun would be a magnetized sphere. The ends of these polarized circulations at the solar surface should develop an outside magnetic field to correspond with the interior. In my early researches of nearly twenty years ago it was shown that the lines in the

solar corona are so distributed, especially at the time of minimum sun spot activity, as to indicate strongly that they were arranged around the *sun as a magnetized sphere.* It is not necessary here to review the many details which pointed to that conclusion. The great objection to that theory at the time in the minds of scientists consisted in the fact that the sun was too hot to be a magnetized sphere. It was pointed out by me that the earth is certainly a magnetized sphere, and that its interior has a very high temperature. Since those days the discovery of the ionization of matter, whereby dynamic forces of one kind or another disintegrate the atoms, of which molecules are composed, into their primal constituents, which are pure charges of electricity, and the demonstration that the free ions, positive or negative, as the case may be, wander about from place to place and produce magnetic field, have made this theory of the sun much more intelligible. The additional discovery of the Zeeman effect of magnetic field in the sun spots greatly strengthens my theory, and in fact it is not easy to see how solar phenomena can now be discussed on any other general basis.

The solar output shows itself in an irruption of prominences, in a very extended corona, and in an *invisible radiation* stretching out to almost unlimited distances in space. The polar magnetic field of the sun, of which the corona is an evidence, will expand to great distances from the center, and its strength may perhaps be detected as far as to the distance of the earth. Electromagnetic radiation stretches out over the solar sphere radially in every direction, a small pencil of the same falls upon the earth in its different positions along the orbit from day to day, and sets the circulation up in the earth's atmosphere which has been described. This solar radiation falling upon the earth's atmosphere is in part absorbed by it, so that the molecules and atoms yield up their ions, which by redistribution produce the observed phenomena of the earth's electric field, and also certain well-known variations in the strength of the earth's magnetic field. The entire subject is full of difficulties, but at the same time it possesses a fascination to the student such as pertains to very few branches of modern science. This same radiation of the sun falling upon the earth produces the temperatures which vary from place to place, from season to season, and from year to year, in a very complex series of changes which, taken as a whole, constitute what is called the earth's climate. There are many indications that this solar radiation, that is to say, the electromagnetic energy which the sun sends forth into space, is not exactly constant. The sun seems to be a variable star, the variation in its heat and light is the natural consequence of the incessant changes of temperature and pressure, in the circulation, the electricity and magnetism, which are going on within the solar mass. We have already been able to show from our studies of the barometric pressure, temperature and vapor pressure in

different parts of the earth, especially of the United States, that there is a definite though complicated synchronism, which connects the variations of the solar action with the variation in the terrestrial climatic effects. This is a large subject which can not be properly undertaken in this lecture. It may be said in general that as the sun gets more energetic in some parts of its period, the temperatures in the earth's tropics are higher, and simultaneously in the temperate zones they are lower. At the same time the barometric pressures in the atmosphere of the earth centered around the Indian Ocean are higher, while in North and South America they are lower. In the Pacific states the temperatures increase with the solar energy, and in the central and eastern states they decrease. The solar impulse which produces these effects tends to precede the terrestrial exhibit which depends upon the solar impulse by some months, possibly by a year under certain conditions, and this anticipation of course promises an opportunity to develop what may become a rational ground for a seasonal forecast for terrestrial weather. The entire field of operations is very complicated, the circulation in both atmospheres tends to mask and make more complex the pure variation of the solar radiation, so that we must be very cautious in attempting to pronounce for or against certain tentative conclusions regarding this subject. It will probably require more than one generation of men to make practicable and popularize the result of this research. Mathematicians as well as laymen are cautioned to withhold negative evidence based upon half understood phenomena, because it is in fact very difficult to disentangle the net which nature has spread before us. The threads should not be torn and distorted by the bungling hands of those who have not the training required to unravel the several skeins which lead to the center of the great mesh. It is certainly not saying too much to assert that there is good ground for proceeding positively and firmly along this line of research, and the fact that it has attracted the attention of many commissions, international committees, scientific societies, observatories and institutions shows to what an extent the great problem has already commended itself to the favor of scientific men.

THE REORGANIZATION OF AMERICAN FARMING

By Professor HOMER C. PRICE

THE OHIO STATE UNIVERSITY

FROM the beginning, American agriculture has been characterized by its extensiveness rather than its intensiveness. Land has been more abundant than labor and, in the aggregate, more has been derived from a small yield on a large acreage than could have been realized from a large yield on a small acreage. The yields of American farm crops have been proverbially small, but the total production has been exceptionally large and, as a rule, the countries producing the largest amounts of farm crops have the smallest yields per acre. This fact is illustrated in the following table:

Table showing the Average Yield of Wheat per Acre by Ten-year Periods for the last Twenty Years and the Total Production for 1908

	Avr. Yield per Acre, 1888-97	Avr. Yield per Acre, 1898-1907	Total Production
United Kingdom	30.1 bu.	32.6 bu.	55,585,000 bu.
Germany	22.7	28.4	138,442,000
France	17.6	20.8	310,526,000
United States	12.8	13.9	664,602,000

The above table also reveals the fact that the production per acre when compared by ten-year periods has been increasing in all the countries. Much has been said and is being written about the decline in agricultural production, but statistics do not show that there has been any decline, but rather a marked increase when the productions of the leading countries are compared and using the production of wheat, which is the most universally grown farm crop, as the basis for comparison.

The intensity of culture always bears a direct relation to the density of population and while it is difficult to get a comparable basis of comparison between countries on account of the varying proportions of waste land in different countries and different methods of classifying statistics, the following table represents the most reliable figures available and, when compared with the preceding table, shows that the yield of wheat per acre varies directly as the density of population.

Number of Acres per Capita

United Kingdom	1.70 acres.
Germany	2.37 acres.
France	3.40 acres.
United States (exclusive of Alaska and Philippines)	24.02 acres.

In 1900 there were 838,000,000 acres in farms in the United States, and since then we have been adding to them about 15,000,000 acres each year from the public lands of the country. During this time, however, the population of the country has been increasing at the rate of about one and one half million each year. The public lands of the country that are suitable for agricultural purposes have practically all been taken up; the tide of immigration has been turned back from the Pacific coast, and the competition for land already under cultivation has become much more keen and, as a consequence, the values of farm real estates have advanced generally throughout the country, but to the greatest extent in the western states. Farm lands in some sections have doubled or even tripled in value in the course of a few years.

Together with the increased value of farm lands have gone other changes that have had an important bearing on the agriculture of the country.

The development of methods of transportation and the extension of railroads through the new agricultural lands have widened the markets of the country, for both buying and selling. The introduction of refrigerator car service has made possible the shipping of fruits, meats and other perishable products across the continent. This has resulted in bringing the products of cheap lands in competition with the products of high-priced land in the eastern states.

Another factor that has had an important bearing in this connection has been the development of labor-saving farm machinery. If the present wheat crop of the United States were harvested by the method employed at the time of the civil war, it would require every man of military age in the United States to work for at least two weeks in wheat harvest. The invention of labor-saving machinery has increased the producing power of the individual to such an extent that notwithstanding the increase in the agricultural exports of the country from $205,853,748 in 1858 to $1,017,396,404 in 1908, the percentage of the population engaged in agriculture has decreased by decades as follows:

1880	44.3 per cent.
1890	37.7 per cent.
1900	35.7 per cent.

But notwithstanding the constant decrease in the proportion of the population engaged in agriculture, the per capita production for the entire population of the most important classes of agricultural products has increased almost invariably.

The following table gives the average per capita production by decades, 1866–1908. These statistics are from the United States Department of Agriculture:

	Decade				Period
	1866–1875	1876–1885	1886–1895	1896–1905	1905–1908
Wheat, bushels	6.2	8.3	7.4	7.8	7.9
Corn, bushels	24.6	30.3	27.5	28.4	31.8
Oats, bushels	6.9	9.1	10.7	10.6	9.8
Barley, bushels	.7	.9	1.1	1.3	1.9
Rye, bushels	.5	.5	.4	.4	.4
Potatoes, bushels	3.0	3.0	3.0	3.1	3.4
Cotton, pounds	37.8	51.4	57.8	65.5	70.0
Hay, tons	.63	.76	.86	.76	.74
Horses, number	.20	.21	.23	.20	.23
Milch cows, number	.25	.24	.25	.22	.24
Other cattle, number	.37	.47	.55	.49	.55
Swine, number	.69	.75	.73	.58	.64
Sheep, number	.80	.84	.68	.63	.64

This almost inconceivable increase in agricultural production has been accompanied by changes in agricultural conditions that make a reorganization of American farming methods absolutely necessary.

Foremost among these changes has been the growth of cities from an urban population of 2,897,000, or 12.5 per cent. of the population total, in 1850, to a population of 24,992,000 or 33.1 per cent. of the total population in 1900. This concentration of the population has brought about new problems of food supply in furnishing the more perishable products such as milk, vegetables, fruits and such products as need to be consumed soon after production.

Another condition that has arisen is the tendency of the soil fertility of the farms of the older agricultural sections to become exhausted. To remedy this, the use of commercial fertilizers has become general in eastern United States and the statistics of 1900 show that $55,000,000 worth of goods were used by the farmers of the United States, which was an increase of 42 per cent. over the amount used in 1890, so that it is probable that not less than $75,000,000 per year is spent for this purpose.

The opening up of the middle west took from the farmer of the eastern states his market for wheat and other grain. He was thrown in competition on the open market with the farmer who had secured his land for practically nothing and land that was much more fertile and productive. The farmer of the middle west, in turn, has been thrown in competition in the live-stock markets with the live-stock products of the western and southwestern states and territories. Stock that was raised under range conditions and often on government land free of charge competed with stock raised on high-priced farms of the middle west.

While these conditions are not so emphatically true as they were a few years ago, yet the problem is far from being solved and the American farmer is now passing through a transitional stage and the most

important problem before him at the present time is the question of reorganizing his farming methods so as to best fit the agricultural conditions as they now exist.

The unprecedented increase of values of farm products in recent years resulting in a greatly increased cost of living to every one has resulted in the most prosperous times the American farmer has ever experienced, except during the civil war by those who stayed at home and reaped the benefits of high prices.

The consumer, on the other hand, is alarmed at the continued rise in price of the necessities of life. He is interested in knowing what the end is going to be and how much longer prices are going to rise.

Writers who are ill-advised of the potential producing power of American farms are freely predicting that we are rapidly approaching the time when as a nation we shall not be able to produce sufficient food stuffs for our own population. They forget that our farms are not producing more than one half of what they are capable of doing. Our average wheat yield is 14 bushels per acre; our average yield of corn is 26 bushels, and of oats, 25 bushels; these yields can and will be redoubled in the future as the high price of the products will demand.

The profits of farming in the past gained from actual production has not been in proportion to the profits derived from other industries. The market price of farm products has tended toward the actual cost of production of the average crop at current wages rather than the cost of production of the part of the crop produced under the most unfavorable conditions. This is readily demonstrated by taking the actual amount of time required to grow and harvest an acre of any of the principal crops and calculating the time at current wages and the average yields at farm prices.

The results will show that the returns received for the time spent will not be more than enough to pay current wages and six per cent. interest on the investment in land and equipment. Farmers have received greater returns from the increased value of their lands than they have from the profits upon their productions.

The increased prices of farm products are beginning to bring to farmers a just return for labor expended and will do more than anything else to turn the city dweller "back to the soil" and to keep the country boy on the farm. There is no danger of a shortage of food supplies in this country, but higher prices must prevail in order to develop the potential agricultural resources of the country. Aside from the possibilities of doubling the present crop production on present area under production, there remains the undeveloped agricultural lands of the country. Aside from the limited amount of land suitable for agricultural purposes still remaining in the ownership of the government, the lands that may become valuable for agricultural purposes are of

two kinds—the swamp lands that may be reclaimed by drainage and the arid lands that may be reclaimed by irrigation. The United States Geological Survey estimates that75,000,000 acres can be made valuable for agriculture by draining swamps. This is the equivalent of one sixth of all the land now under cultivation in the United States. This land would be much more fertile and much more productive than the most of the land that is now being cultivated. The reclamation of arid lands is just in its infancy. The first federal act to provide for government assistance for this purpose was passed in 1902.

Projects are now under construction or have been completed that will reclaim one and a half million acres and others are under consideration that will reclaim three and one half million. To what extent this work of reclamation will be carried in the future can scarcely be estimated, but doubtless many millions of acres can be and will be added to our cultivable lands in the future.

The period of low prices for farm products and extensive methods of farming is rapidly passing. The large grain and live-stock farms of the eastern states are giving way to the smaller dairy, fruit, vegetable or poultry farm. The large wheat farms of the northwest are being divided into moderate-sized farms for mixed farming. The ranges of the west and southwest are being broken up into stock farms and the movement everywhere is toward more intensive methods of farming.

The problem that now confronts the American farmer is to reorganize his method of farming so as to adapt it to the present conditions. The increased prices for farm products will increase their production and insure a supply sufficient for all needs for the future.

INSECTS AND ENTOMOLOGISTS, THEIR RELATION TO THE COMMUNITY AT LARGE. II

By Professor J. B. SMITH

RUTGERS COLLEGE, NEW BRUNSWICK, N. J.

AND now, having given a very hasty and superficial statement to show how important a place the insect really occupies in the social economy, it behooves me to say something of some of the men whose labors made some of these facts and conclusions known.

Many of the matters of which I have spoken are of recent development and the men who have done the work are still with us and still working. Some are in attendance at this very meeting and as we expect still better work from them, nothing will be said of what they have done thus far. And while it is intended to confine the mention to American entomologists, it is necessary to include under that head some whose claim to be called American rests altogether upon the work done with or on American insects. Let me say too that the order in which the names come is not meant to represent anything more than convenience in arrangement of topics, and finally, it is not to be understood that omissions show lack of regard, but only that within my time limit no photographs were obtainable.

Thomas Say has been termed the father of American entomology, and certainly no one is better deserving of that term than he. He builded well and broadly and his knowledge of the American insect fauna was surprising. His work was in all orders and the amount of material that passed through his hands was very large. Unfortunately most of his types have been destroyed, so that we are not now able to see the specimens that he had to work with. This has made less trouble than with some other authors, because Say had that wonderful faculty of seizing upon and describing the specific peculiarity of the individual before him. I well remember the hours that I spent over his descriptions, trying to identify captures made thirty-five years ago, and while I was often disappointed, I succeeded in correctly identifying what I now consider a really large percentage of the forms taken. Say's experience meant hard work under difficulties: no money—very little literature. His bed, for a time, the floor of the Exhibition Hall of the Academy of Natural Sciences in Philadelphia, his food costing six cents per day. Encouragements there were few—discouragements many and none greater than the lack of literature. None of the younger men can appreciate that hunger for books with which the older men were compelled to fight and the enjoyment of getting into an alcove with

GEORGE H. HORN

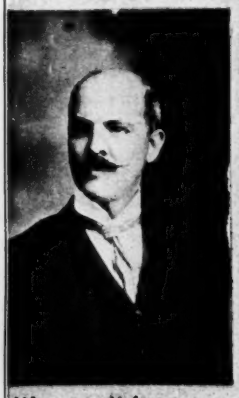
SILAS S. HALDEMAN

JOHN L. LE CONTE

BARON R. VON OSTEN-SACKEN

THOMAS SAY

THADDEUS W. HARRIS



WILLIAM H. ASHMEAD

HERMANN LOEW

H. A. HAGEN

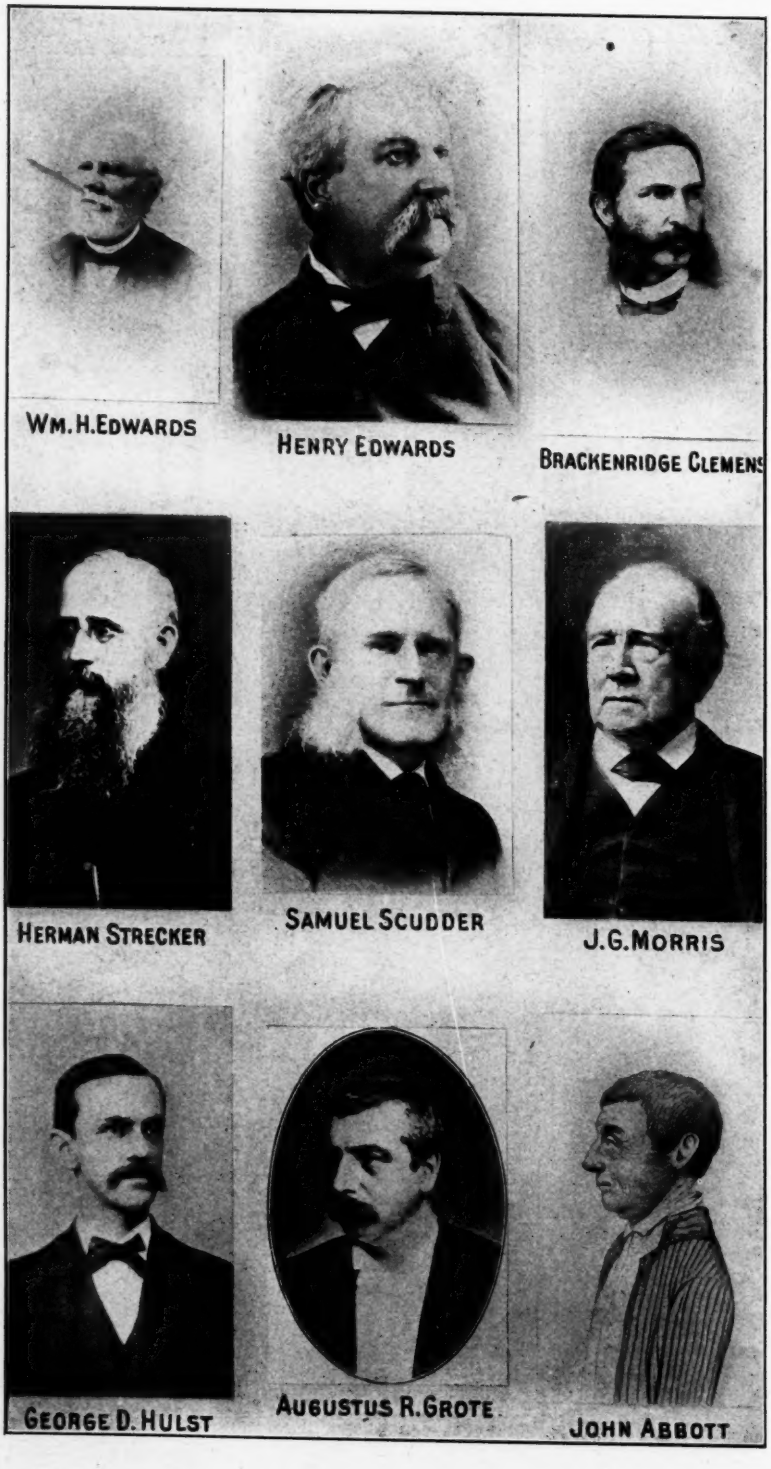
volumes that you knew were in existence but had never before seen. And yet Say was well off in these matters because he had the library of the academy to draw upon, and there were then—possibly there are now—more works on entomological subjects in Philadelphia than elsewhere in the United States.

MELSHEIMER AND HALDEMAN were also of the Philadelphia clan, coleopterists and systematists, and to the former we owe the first catalogue of American coleoptera—an excellent piece of work for its time and of very great use to students until it was superseded by the Crotch check list, which remained the standard for many years until it in turn was superseded by the Henshaw list. These latter check lists are both the work of the Boston circle, Crotch having done most of his work at Cambridge, where Henshaw is still doing excellent service.

DR. JOHN L. LECONTE, of Philadelphia, has without doubt done more for American coleopterology than any one other man. It was my privilege to know him personally and to profit to some extent by his encouragement and advice. Dr. LeConte, though confining his work to the coleoptera, was by no means narrow in his knowledge, and the comprehensive view that he was able to take of his subject is witnessed in the "Classification of American Coleoptera," which forms to the present day the basis of our knowledge in this order, and which will maintain its value though the order of families may be changed and their relationships better established. Dr. LeConte's collection is now at Cambridge, accessible to all serious students.

DR. GEORGE H. HORN, first a pupil, later a collaborateur with Dr. LeConte, did as much or even more systematic work in coleoptera. But the work is different: Dr. Horn was a genius in the separation of species and in their arrangement within generic or family limits; but he lacked the broad views of Dr. LeConte and was more precise in working out details. With Dr. Horn I was well acquainted, and many an hour did I spend in his room among his boxes, while he was on his rounds; for the doctor had a large practise and entomology was his recreation. I regret that I can not give a picture of that room. There was a cot in one corner which was often the only available place to sit; there was a huge table or desk occupying most of the floor and, during the many years that I knew that room, this table was cleared only once. Occasionally the cigarette stumps would be gathered together and thrown out; but the dust and dirt were never otherwise disturbed. Cabinets and book-shelves were about the walls and books were everywhere—on the floor, the chairs and often even on the bed. It was strictly a workroom and the doctor was an indefatigable worker. His collection is now in the rooms of the American Entomological Society in Philadelphia.

JOHN ABBOT, associated with J. E. Smith in the work on the rarer



WM. H. EDWARDS

HENRY EDWARDS

BRACKENRIDGE CLEMENS

HERMAN STRECKER

SAMUEL SCUDDER

J.G. MORRIS

GEORGE D. HULST

AUGUSTUS R. GROTE

JOHN ABBOTT

lepidopterous insects of Georgia, is an example of the combination of artist and entomologist, and his *published* drawings give no idea of the amount of the work he actually did, nor any real idea of its beauty and accuracy. There are bundles of unpublished drawings in the British Museum, a few of them in the Boston Society of Natural History, and others scattered about. Some of the insects figured have never been found since; some, described from the figures by Guenée, Boisduval and others have never been satisfactorily identified, and I well remember my hunt through Paris, over twenty years ago, under the guidance of M. Aug. Sallé after the original of one of Boisduval's descriptions, which was finally located in the possession of a former housekeeper, who fell heir to some of the effects of her master.

A hale, hearty old man was DR. J. G. MORRIS, of Baltimore, when I first met him thirty years or more ago, and never was I more pleased to meet any one because, somehow, I had received the impression that he was dead. Dr. Morris made the first attempt to gather together the descriptions of American lepidoptera, and his volume in the Smithsonian series proved a very useful one to the collectors of that day. Unfortunately the scheme was never completed, and a very small section only of the Heterocera is represented in the volume. Dr. Morris did not, I believe, ever describe either genus or species, and never pretended to any extensive collection.

A. R. GROTE, of Buffalo, and later, New York, was a most earnest worker in the heterocerous lepidoptera and chiefly in the Noctuidæ. To him we owe the first satisfactory arrangement of our species, and the identification of the species described earlier by Guenée and Walker. It was no light task, and how remarkably well done it was I did not realize until years thereafter, when I undertook similar work. Mr. Grote's collection is now in the British Museum, where I have had the opportunity of comparing its types with those of Walker and Guenée which are also in that rich treasure house.

MR. W. H. EDWARDS, of Coalburg, W. Va., I never met, although his death is comparatively recent. But his magnificent work in the butterflies lives on, and will continue to live. Mr. Edwards was much more than a describer of genera and species. He was a real student of the life of the insects, and he did more to make known their early stages than any one other worker: and besides, he set up a standard of thoroughness and accuracy, that our younger students must live up to if they expect their work to be regarded. His collection is now in the Carnegie Museum, at Pittsburgh.

MR. HENRY EDWARDS, of New York City, was one of the centers of entomological interest in that city—hearty, whole-souled, enthusiastic. He made friends wherever he went and his travels carried him not only throughout our own country, but into Australian and Asiatic countries

as well. He was an excellent collector and his cabinet was unusually rich in Californian and Pacific coast forms. This collection remains in New York, and forms the nucleus for the lepidoptera in the collection of the American Museum of Natural History.

Dr. George D. Hulst was one of the Brooklyn entomologists and devoted his energies to work in the lepidopterous families Geometridæ and Pyralididæ. While his systematic work in these groups is most useful, it is not equal to his personal influence upon those that were fortunate enough to come into contact with him. I grew to love that man and felt his death as a personal loss. His collection is now in my charge at Rutgers College, to which institution it was given by him before his death.

Dr. Herman Strecker, of Reading, Pa., was known to many of our older members, and never were there more diverse judgments than those passed upon him. But he was earnest if erratic, and succeeded in accumulating an enormous collection of lepidoptera during his long life. He would pay any price for a specimen that he wanted, and halt at no expedient to secure what he could not buy. He was a genius with pen, pencil and chisel; a sculptor of mortuary emblems by profession, and a painter of butterflies by choice. His publication on this subject was unique: all the drawings and engravings were made by him, and all the plates were hand colored. His industry was continuous and he was tireless in his work. His writings were spicy and he never hesitated in printing what he wanted to say: he was his own publisher and had none to say him nay. His collection is now in the Field Museum in Chicago.

In the tineid families of the Microlepidoptera there was an immense untilled field which only one of the older American students had the courage to undertake. To Brackenridge Clemens belongs the honor of breaking ground in this series, and upon his work the subsequent students in the group, whom fortunately we yet number among our associates, have built their own. Clemens also did some work in the macros, notably the Sphingidæ, and many of his types are yet to be found in the collections of the American Entomological Society.

Among the unique figures in American entomology none looms larger than Dr. H. A. Hagen, of Cambridge. Big, ponderous, thoroughly German to the end of his life, intensely loyal to his chief and his work, he was easily the most learned entomologist of his day. His monumental work in the literature of entomology has proved a gold mine for later students, and would alone have been considered a creditable life work. But Dr. Hagen was also a special student in the Neuroptera, and his volume in the Smithsonian series is essential to every student in the order to the present day. I knew Dr. Hagen well and was his guest at times. I won his heart by the meekness with

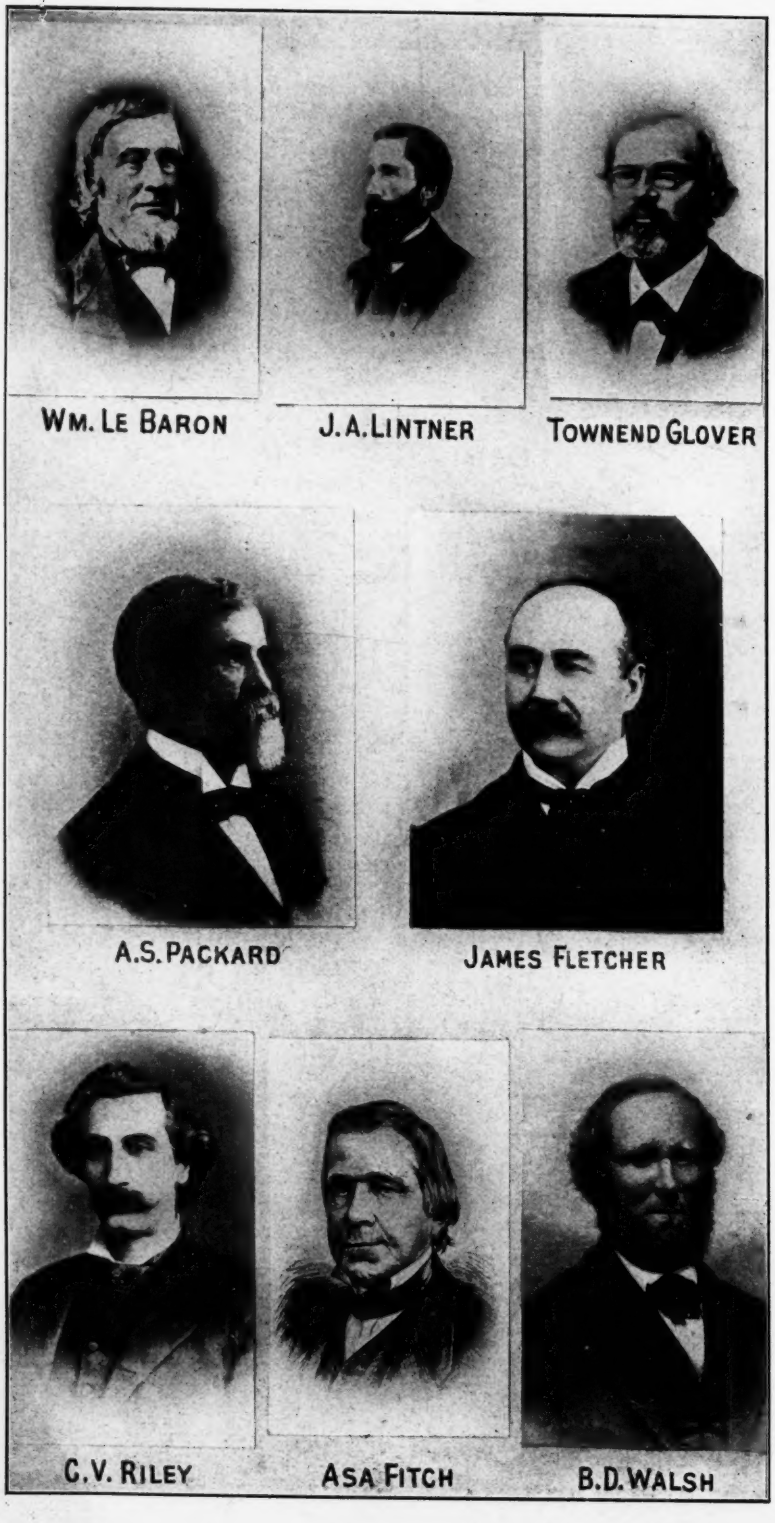
which I accepted a severe reproof concerning a sending of diptera for determination. He had kindly replied to a letter of mine asking for aid and, in return, I had packed a cigar box as full as it would hold of undetermined specimens, big and little. I got it back next mail, and with it a letter. The letter was instructive, very—and if the medicine was bitter, it was at least salutory for I never did the like again, and have never dealt quite so hardly as I might with those who have in later times imposed upon me, as I did upon Dr. Hagen. His library and his collections are at Cambridge, and no one who has not seen both can appreciate the amount and character of the work that the good doctor did during his lifetime.

Baron von Osten-Sacken was an unusual combination of diplomatist and entomologist. Of his standing in the former capacity I know nothing; as a dipterist none stands better. To him we owe the early systematic work done in this country and the series of volumes published by the Smithsonian Institution, for even the work of Loew was made available through translation by Osten-Sacken. And so these two pioneers of American dipterology must almost necessarily be considered together, although the influence of Loew could not be so great because of his dependence on a translator in reaching the American public. Shortly before his death Osten-Sacken published his memoirs, which certainly make interesting literature. A large part of his collection is in the museum at Cambridge.

Among the hymenopterists I can mention only William H. Ashmead, whose death is so recent that most of us remember him personally, and whose gentle manner and unfailing courtesy endeared him to all who came into contact with him. His work was monumental and his systematic sense so developed that he seized almost at a glance upon the really essential structures of the species studied by him. So constant and persistent a worker was he that, to those of us who knew him personally, the surprise was not that he died so young, but that he lived so long.

Dr. A. S. Packard, of Brown University, was more than an entomologist: he was a biologist and a teacher. His work as a systematist was great; but as a teacher he was greater. And his teaching was not confined to the classroom; his "Entomology for Beginners," his "Guide to the Study of Insects" and his "Text-book of Entomology" continue his work, though his voice in the classroom is hushed. His interests were broad enough to include even the economic side of the subject and he appeared as a member of the U. S. Entomological Commission, though his part of the work was that which was more technical in the publications.

I can scarcely avoid referring at least to Dr. S. H. Scudder, although he is yet with us, not only because his work, unfortunately,



WM. LE BARON J. A. LINTNER TOWNEND GLOVER

A. S. PACKARD JAMES FLETCHER

C. V. RILEY ASA FITCH B. D. WALSH

is done; but because it was carried into fields not theretofore frequented by American entomologists. His labors on the fossil insects of America are unique, and his collection of material for further work is immense. Of his systematic papers on orthoptera and his accomplishments in other directions I will not speak at present.

All these men paved the way—they made the studies necessary to familiarize us with the insects round about us, and theirs is the labor that is not spectacular and whose apparent results are not of public interest: yet such work we must have as a foundation for what we consider the more practical side of the subject.

First among the economic entomologists of this country we must reckon DR. THADDEUS WILLIAM HARRIS, whose work on the "Insects Injurious to Vegetation" is a classic and, like most of the classics, was a labor of love rather than a money-making proposition. The state of Massachusetts paid him $200 for that work. Since that time it has learnt to pay rather more highly for entomologists, and nowhere have insects done more injury nor have they anywhere demanded the expenditure of greater sums. Harris's work is not only intensely practical, but it is interesting and informing—as useful to the beginning collector and entomologist as to the agriculturist, and always accurate.

Quite a different sort of man was TOWNEND GLOVER, for a series of years entomologist to the U. S. Department of Agriculture, who wrote comparatively little, but used his pencil industriously; producing a perfectly enormous number of drawings of insects in all stages, and engraving them on plates from which only a very few impressions were taken. Unfortunately, Glover had almost no systematic knowledge of insects, and while he made excellent pictures of the specimens as they appeared to him, he had not the slightest idea as to the identity of the insects figured, nor did he preserve the originals.

DR. ASA FITCH, of New York, was a man of different type. A hard worker and hard student, industrious, of course, he studied not only those species that his field work demonstrated to him as injurious, but their allies and neighbors, and with a sure glance he fixed upon certain of the hemipterous families as entitled to the special consideration of the economic worker. Dr. Fitch's reports as state entomologist initiated a work in that state which has not been abandoned since, and which has put it among the leaders in organizations for entomological work.

Meanwhile, in the middle west the ravages of insects had developed new needs and new workers, and Walsh, Riley and LeBaron, began to make themselves felt, and really to develop a science of economic entomology.

BENJAMIN DANN WALSH, of Illinois, had a career much too short, and it terminated before he had more than shown his vigor and orig-

inality. In company with Riley, then a young man, he had planned much in the way of entomological work; but one report and a few vigorous papers in journals form the total of what remains to us.

Dr. William LeBaron, his successor in office, was a much less positive character, but an equally conscientious worker, and, in his fourth report, began what was intended to be a popular treatise on the insects, the systematic portion forming a sort of supplement to the specially economic portion. Illinois is another of the states which has never allowed its service to deteriorate, and there is no better work now done in the United States, nor is there a more effective organization than that within its limits.

Dr. Charles V. Riley was a prime factor in the development of economic entomology in the United States. His series of reports on the injurious and other insects of Missouri is a model which has never been exceeded in interest and value. Not the least important feature of these reports is the list of illustrations—wood-cuts most of them—that have never been surpassed in their fidelity to nature, and their artistic merit. Most of the insects figured in Riley's reports *look* natural, and that is the highest praise that can be given to any figure of this type. So well done are they that they have become common stock and are used again and again in bulletins and reports throughout the country. With his transfer to Washington his field of activity was enlarged, and he became a force in the development of the practical side of entomological work. The real development of our present battery of spraying outfits, arsenical poisons and kerosene emulsions began under Riley, and the fight to secure their adoption was a more difficult one than is understood. Congress thought itself very liberal when it reached the $20,000 mark for the division of entomology, and when we consider the force of men that Riley gathered and trained for that sum, men who form the nucleus of the division to the present day, we begin to appreciate the ability of the man.

I will not attempt to give a list of the men who were associated with Dr. Riley in the development of his office at Washington; I knew them all and worked with some of them for a time. And not the least of Dr. Riley's ability was in getting all that there was out of his assistants, in commanding their devotion and loyalty, although he constantly quarrelled with every one of them. He was the best loved, best hated, most admired and most detested man I ever knew; but he was always a better friend than he was an enemy, and never lost an opportunity to do a man a good turn even when he personally lost by it. Economic entomology owes much to Dr. Riley and his influence is still with us. I need hardly say that his successor has fully maintained the standard set for him, and that there is nowhere in the world at the present time a more efficient body of workers in economic ento-

mology than that connected with the U. S. Department of Agriculture at Washington.

DR. J. A. LINTNER, who was for many years state entomologist of New York, was a model of gentle, persevering labor. Quiet and deliberate in manner, very painstaking in his work and observations, he maintained the high standard set for his office by Fitch, and his reports are models of completeness in the treatment of the subjects contained in them. He was a familiar figure at the meetings of the American Association for the Advancement of Science, and was always listened to with respect.

Last of all in this list of those who have been influential in the development of the fight against insect pests, because his loss is one of the more recent, is DR. JAMES FLETCHER, of Ottawa, Ontario. Who of the entomologists attending the annual meetings of the American Association does not remember his hearty and cheering presence. Who does not remember his cordial greeting, his constant good nature and the directness and convincing qualities of his contributions to our discussions and debates. As for the work that he did in Canada —none could have done it as he did. He was widely informed, not a narrow specialist, he was a student of men as much as of insects, and he commanded the confidence of his constituency. It will take two men or more to carry on the work that this one did alone.

To summarize—insects are a factor of very great importance in the community: (1) because of their injuries direct and indirect; (2) because of their benefits, also direct and indirect, and millions of dollars annually are involved on both sides of the ledger.

The entomologist who studies these insects, determines which are harmful and which are beneficial, who works out their life histories and habits and who determines methods of controlling those that are harmful and improving those that are beneficial, is a worker of high importance to the community and deserving of every possible aid and assistance.

ANCIENT CLIMATES OF THE WEST COAST

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WE naturally look at things from the standpoint of the present, and regard the existing distribution of climates as the normal one. But even in our own times there are slight fluctuations of climatic conditions, for we hear wonderful stories from our elders of cold winters and hot summers, and tremendous storms of former years. The advances and retreats of existing glaciers give us surer testimony of recent fluctuations in temperature and moisture, as does also the shifting of the zones where wine grapes can be grown successfully in Europe.

If we go still further back into older history we find still stronger evidence of change, for in northern Africa and in central Asia there are remnants of ancient cities, evidently the flourishing capitals of prosperous peoples, where now is nothing but desert, and where even the most advanced modern skill in irrigation could not support the population of the old days.

And yet all the changes alluded to above may be only the secular variations in climate that we know are going on all the time. The climatic changes of the west coast which will be described in this paper are older than those fluctuations recorded in history, and much greater.

The old geological theory was that the earth cooled down slowly from the poles toward the equator, and that life first appeared at the poles. It was further thought that in the more remote geologic ages the interior heat of the earth was so great that there was little difference in temperature between the equator and the poles, and that, until Tertiary time, there was no differentiation into climatic zones. The Glacial epoch was supposed to be the culmination of this secular cooling off of the earth.

Then came the discovery of the old Tertiary fossil floras of Siberia, Alaska and Greenland, with abundant forests of trees that evidently lived in a temperate climate where it is now arctic. This was so remarkable that geologists had to invent some extraordinary explanation for the phenomenon. They rose to the occasion and invented the theory of the obliquity of the poles in early Tertiary time, to account for the warm temperature under the arctic circle. This, however, did not agree with the known distribution of life at that time over the rest of the earth, and also the physicists declared this obliquity, or any obliquity, to be a mathematical impossibility.

Later it was discovered that there was a great glacial epoch in the

late Paleozoic era, about the line between Carboniferous and Permian, and that too in the regions around the Indian Ocean, where it is now tropical or subtropical in temperature.

Before we had entirely recovered from the shock of that discovery, it was found out that in China and Australia there was another glacial epoch, or epochs, near the beginning of Cambrian time. Now the geologist's spirit is so broken, that when the supposed discovery of glacial epochs in Silurian and Devonian time is announced, he hardly raises a dissenting voice, and appears to be resigned to the occurrence of glacial epochs at almost any time in the history of the earth. The theory of ancient climatic uniformity is definitely abandoned, and we must accept fluctuations of climate from the earliest geologic record all through the history of our planet. The old idea was delightfully simple, but too good to be true.

Criteria of Ancient Climates

Physical Criteria.—Physical evidence as to ancient climatic conditions is limited to two classes—glacial deposits and ice-work, and sediments indicating desiccation, that is, saline and gypsiferous beds. These are both necessarily limited to continental areas, and tell us nothing of marine conditions. And as we go back in time they become more and more indefinite, so that there is much difference of opinion as to their value. The evidence of the recent Glacial epoch is positive enough to satisfy the most critical, but geologists are not yet united as to the glacial epochs in older periods of geologic history, because of the difficulty of determining whether the ice-masses were true sheets or whether they were mere local highland glaciers.

Also the sedimentary deposits indicating desiccation may have been merely local, and although they are positive as to prevalence of evaporation at that particular place, they can not tell positively of wide-spread dry climate, and certainly they do not indicate temperature.

Organic Evidence.—Fossil remains of animals or plants known to have lived in either warm or cold climates are more definite, and tell us equally well of land and water conditions, but they are authentic only when the fossils are animals or plants that have either lived on into our own time, or when the groups to which they belong have always had the same habits. This becomes more and more conjectural as we go back in geologic history, and have to deal not only with extinct species, but even with extinct genera, families and orders.

Extensive fossil beds of deciduous trees point to moist climates, and usually to temperate conditions. But deciduous trees extend back only to the middle of the Cretaceous, and beyond that time we have no positive criteria for temperate climate.

Cycads and palms are the best evidence as to tropical climates on

the land. At present, cycads are almost exclusively tropical, ranging outside only a short distance in eastern Asia. Palms are not quite so delicate, ranging outside the tropics to 34° N. lat. on the west coast of America and to 36° N. on the east coast. But in any case, abundant remains of either point to tropical or subtropical conditions.

Reef-building corals are even more definite in their testimony concerning tropical temperature of the water. They are now found only in the tropics, where the winter temperature does not fall below 68° F. (20° C.), and in general between 26° N. and 26° S. But since this temperature zone may be extended by marine currents, coral reefs may sometimes reach beyond 26° N. lat., as in the Bermudas, but more often they fail to reach this geographic limit, as on the west coast of America.

The principal reef-builders, the Madreporidæ and the Astræidæ, are confined to the hottest part of the tropical belt,[1] within 18° of the equator, and where the temperature does not fall below 74° F. (23° 20′ C.). Between this line and the isotherm of 68° F. coral reefs occur on both sides of the equator, but they are composed largely of Poritidæ and Milleporidæ.

On the west coast of America the minimum isotherm of 68° F. runs north of the equator, and the Galapagos Islands have no reefs, for the temperature there often falls below 68° F. Reef-building corals occur in patches from Panama to Magdalena Bay on the coast of Lower California, but they do not form any reefs, and are composed almost entirely of Poritidæ.

Fossil deposits of Astræidæ, in any age and anywhere, indicate with a reasonable degree of certainty that the sea had a temperature of not less than 74° F., and corals of any of the modern reef-building groups show that the temperature was not less than 68° F.

But the reef-building Hexacoralla are not known below the Triassic, and for the Paleozoic era we must use other criteria. From the Cambrian to the upper part of the Carboniferous coral reefs are known, but they are formed by Favositidæ and Tetracoralla, both wholly extinct, so that we can only infer their habits. It is, however, nearly certain that these ancient reef-forming corals lived under the same conditions as the modern groups, and that the temperature of the sea where they lived was tropical.

Absence of coral reefs from any formation does not prove that the temperature of that time was not tropical, for even now coral reefs are lacking in many parts of the tropics, on account of unfavorable conditions other than low temperature. Also the corals of the ancient reefs have often been obliterated by metamorphism, and only massive limestone left.

[1] J. D. Dana, "Corals and Coral Islands," 3d ed. (1890), pp. 108–114.

Now while one swallow does not make a summer, one reef-building coral, or one palm, or one cycad does, since neither one of these organisms now lives outside of a warm climate.

Paleozoic Climate of the West Coast

All the Paleozoic sediments on the west coast are marine, and while the record is fragmentary, the evidence points uniformly to warm temperature of the sea, and, thus by inference, of the land. The Lower Cambrian, or Pre-Cambrian, glaciation of China and Australia has not been recognized in this part of the world, but this is merely negative, since land formations of that period are unknown here.

The Lower Cambrian limestones of Inyo County, California, and the adjacent region of Nevada, have extensive coral reefs of Archæocyathidæ; similar reefs are known in Europe and Australia, but not in the Arctic region.

In the Silurian of Plumas County, and the Devonian of Shasta County, California, there are coral reefs composed of Favositidæ and Tetracoralla, and in both these ages similar reefs are known in Siberia and Alaska, which may show that the temperature of the sea had grown warmer in the middle Paleozoic, with a northward extension of the isotherms.

The Carboniferous of Shasta and Plumas counties, California, has great limestone masses full of reef-building Tetracoralla, and similar reefs are known up to 82° N. lat., and down to the equator. Whatever the temperature was, it was remarkably uniform. The flora of the Coal Measures[2] in the northern hemisphere indicates a warm and equable climate for the land, extending up into the Arctic region, and without evidence of any trace of climatic zones.

The Permian, or Upper Carboniferous, glaciation, which was so widespread in India, Australia, South Africa and South America, has not been recognized in North America. But this event is now recognized as the greatest catastrophe in geologic history, and its effects probably extended far beyond the limits of glaciation. With the accompanying lowering of oceanic temperature, near the end of the Paleozoic era, the ancient types of reef-building corals, the Favositidæ and Tetracoralla, disappeared. Hardly anything but solitary corals, that may have been deep-water forms, are left in the Permian, and in the Lower Triassic no corals of any sort are known.

The Hexacoralla, the modern reef-builders, had already originated in the Paleozoic, but were then little developed, unspecialized types. They escaped the general catastrophe either by being distributed in regions where the destruction did not take place, or by being then deep-

[2] David White, *Jour. Geol.*, Vol. XVII., No. 4 (1909), p. 338.

sea forms, habituated to lower temperatures. When the amelioration of the earth's climate took place, near the beginning of the Mesozoic era, they found a free field on the coasts, and at once took possession. In the epoch of the Middle Triassic they had already become widely distributed, but as yet had formed no known reefs.

The distribution of the cephalopods in time shows a strong contrast to that of the corals. There is an unbroken genetic series of ammonoids and nautiloids from the Coal Measures, through the Permian, and extending into the Lower Triassic, several genera ranging through the interval. This does not necessarily mean that the cephalopods were hardier, for they probably were not. But they were very widely distributed, and must have lived on in some region, or regions, where great catastrophe had little or no effect, and by their superior facility in locomotion got back into the regions affected by glaciation, when the temperature of the seas had risen again.

Mesozoic Climates of the West Coast

Since corals are wholly unknown in the Lower Triassic, and since the flora of that epoch is as yet little known, it is not possible to determine the temperature of either the land or the water. It is, however, certain that the oceanic temperature in India, in western America and in northern Siberia, was the same, for there is a remarkable similarity of the cephalopod faunas in all three regions.

It is also known that in the Permian and the Lower Triassic a dry climate prevailed over large areas, for products of desiccation, such as gypsum and saline deposits are common in many parts of the world, and even in regions that are now rainy, as in western Europe.

In the Upper Triassic there are great limestone masses and coral reefs in the Alps, the Himalayas and in California, with many species common to the three regions. Certainly the epoch of the *Tropites subbullatus* fauna was tropical up as far as Shasta County, California, for there reefs of Astræidæ are extensive. We may even be justified in assuming that the isotherm of 74° F. extended that far north. Also in the Blue Mountains of northeastern Oregon there are coral reefs in the Upper Triassic, but no Astræidæ were found in them, only extinct genera. This outlying occurrence may correspond to the isotherm of 68° F., in which now corals may form reefs, but Astræidæ can not flourish.

After the formation of the coral reefs in northern California and Oregon the facies changed suddenly from limestones to clay shales, and with this came an abrupt change in the marine fauna. The Indian types of cephalopods disappeared entirely, and in their stead came in a fauna of which the home seems to have been the boreal region. *Pseudomonotis ochotica* was the commonest species in this fauna, and was

widely distributed around the North Pacific. It has also been found as far south as Peru, on one side, and down to the equatorial part of the Indian Ocean on the other. This wide dispersion does not necessarily mean a lowering of the oceanic temperature during this epoch, for this species may have lived in deep water, and therefore could easily find uniform temperature from the equator to the Arctic region. But the sudden change of facies and impoverishment of the fauna over such an enormous area are suggestive. A slight drop in temperature, below 68° F. would account for it.

The last epoch of the Triassic, the Rhætic, has no marine faunas anywhere in America, but the flora, with its abundant cycads, is widely distributed in both the northern and the southern hemisphere. Coal deposits are common in this epoch, and this points to a very uniform and mild climate far beyond the present temperate zones.

At the opening of the Jurassic period we find a Mediterranean marine fauna established in western America; this same fauna also extended from the equatorial regions to Alaska, so that we are without evidence as to climatic zones, and can only infer that the temperature was uniform.

In the Middle Jurassic reef-building corals lived in the waters of the Great Basin Sea, and their remains are quite common in Plumas County, California, but in that province they formed no reefs, for the waters were not clear, and much disturbed by the deposition of volcanic ash. Abundant cycads lived on the land in California at this time, adding their testimony to the warmth of the climate. This same Middle Jurassic marine fauna extended up to Queen Charlotte Islands, and to southern Alaska, in the latter place with cycads interbedded with the salt-water fossils. Here, as was often the case, the cycads extended some distance north of the corals, a coral reef with Astræidæ being known in this epoch on Queen Charlotte Islands, in 53° N. lat., while cycads occur as far north as 57° N. lat. In this same epoch the northern limit for coral reefs in the Atlantic region was 53° N., in southern England, while the other invertebrates and cycads ranged up to 80° N. lat. A mild climate must have extended up nearly to the pole.

The Upper Jurassic of California shows a sharp contrast to the preceding epoch; its marine fauna is scanty, and what little there is belongs to the boreal type, the *Aucella* fauna, which is characteristic of Russia, northern Siberia and Alaska. For a short time this fauna ranged down into the edge of the tropics in Mexico. This does not mean that the climate was cold, but merely that the temperature was lower than that at which reef-building corals and the other sensitive invertebrates could flourish. In the Lower Cretaceous we find the same boreal type still persisting as far south as middle California. But here,

as in the Upper Jurassic, the evidence is conflicting, for cycads are known in both formations.

In the Lower Cretaceous epoch there was a sharp contrast between conditions on the Pacific and those on the Atlantic side of America. In the Atlantic waters coral reefs extended as far north as Texas, while no corals at all are known in the Pacific waters of America in California. In the Upper Cretaceous, on the other hand, coral reefs extended to Ensenada, Lower California, lat. 31° 30′ N., while in the Atlantic waters they did not reach so far north. In other words, the Pacific waters on the western side of America became warmer in Upper Cretaceous time than they were in the preceding epoch, while in the Atlantic the conditions were reversed, as was the case also in southern Europe, where coral reefs extended much further north in the Lower Cretaceous than they did in the Upper Cretaceous.

The change in faunal geography in western America about the middle of the Cretaceous period is very remarkable. The Knoxville epoch had a boreal fauna, while with the opening of the Horsetown epoch the facies changed rather abruptly, and an Indian fauna came in. Swarms of ammonites of Indian type occupied the shallow marginal sea, showing at least a great change in geographic connections, if not in climate. It has been suggested by the writer that the opening of the Bering Sea passage during the Mariposa epoch of the Upper Jurassic and the Knoxville epoch of the Lower Cretaceous would account satisfactorily for the change of facies and the lowering of the temperature at that time. The closing of this passage near the end of the Knoxville epoch explains the change of facies from the boreal to the Indian type of fauna, and also the accompanying rise of oceanic temperature on the coasts of western America.

The favorable conditions, inaugurated in the middle of the Cretaceous, continued throughout the Chico epoch, during which coral reefs extended up to Ensenada, Lower California, N. lat. 31° 30′, and a warm climate prevailed even in Alaska. Reef-building corals extended up to the middle of California, but they formed no reefs, since there were no stretches of clear sheltered waters in which they could flourish.

Neozoic Climates of the West Coast

The Eocene climate of the west coast was nearly the same as that of the Upper Cretaceous. The marine deposits have numerous molluscan genera that are now confined to the tropics, and on the land palms abounded in California, Washington and Alaska. No reef-building corals of this age are yet known anywhere on the west coast, and it is probable that the marine temperature was slightly below that necessary for their existence in this region. The climate of the coast, from California to Alaska, was probably very much like that of the

states bordering the Gulf of Mexico. There to-day many tropical molluscan genera are found in the waters, and on the marginal coastal plain there is a mixture of palms, deciduous trees and conifers. This is just what we find in the fossil Eocene flora of California and Puget Sound; laurels, figs, sycamores, chestnuts, elms, liquidambar, oaks, palms and sequoias lived together. From this association we should infer that the climate of the west coast was no longer tropical, but subtropical, and very rainy.

The middle Tertiary faunas are very like the present in the association of genera, and the flora on the land agrees with this. The palms have disappeared, but laurels still occur. It is probable that the climate of the upper Miocene had about the same temperature as that of the present in California, but it had, apparently, a much greater rainfall, or one much more evenly distributed.

The Tertiary flora of the west coast was immensely richer than the present. No elm, liquidambar, nor true laurel lives wild on the west coast now, and many other types that flourished here are gone. The impoverishment of the present tree flora of California, as compared with that of the Tertiary, has been ascribed to volcanic activity, but this is absurd. In the first place the great extinction of the old types took place in the lowering of temperature near the end of Eocene time, while the era of great lava outbursts on the west coast was after the middle of the Miocene. The climate continued to cool off in the Pliocene, as is shown by the northern types of mollusca that then ranged as far south as Los Angeles, and by the freshwater lake deposits of middle California, which contain a fauna at present confined to the Klamath region of northern California and southern Oregon. The flora of the Pliocene in California is very scanty, composed largely of willows, alders and conifers, very much like that of the Olympic Peninsula in Washington.

The constantly decreasing temperature throughout the Tertiary is sufficient to account for the reduction of the flora. The tropical and finally the warm-temperate types were killed off locally, and such as were confined to this region were wholly extinguished. Some of the forms that lived in more favored regions to the south returned after the Glacial epoch. But most of the region to the south of California is not favorable to the extensive growth of forests, and many of the types have never returned to California, except when brought in by man.

In the early Quaternary there were extensive ice-sheets in the Sierra Nevada, and probably the climate of the sea-coast was cool. The glaciers came down the slopes to a line that is now about 3,500 feet above sea-level; it is thought, however, that California stood considerably higher than now, and that conditions here were more like those of the present on the Olympic Peninsula.

After the Glacial epoch was past the climate became warmer, and many mollusca crept slowly up the coast, from the warm waters of Lower California. This southern type reached as far north as Santa Barbara in the upper San Pedro epoch of the Quaternary, during which time the sea probably had a temperature as warm as it now is on the shores of Lower California.

This warming up of the west coast was no mere local phenomenon, for the same thing occurred at the same time on the eastern coast of America, when a warm-water fauna ranged up to the Champlain district. And also in Europe the climate after the Glacial epoch was, for a little while, warmer than it is at present. After the San Pedro epoch on the west coast, and the Champlain in the east the climatic conditions became approximately what they now are, although it may well be that the Terrace epoch had a larger rainfall than that of the present.

Summary

In the foregoing pages it will be noted that during all the known Paleozoic the west coast enjoyed a warm and probably tropical climate, with some suggestion of a northward march of the isotherms, reaching a culmination in the Upper Carboniferous. There is then some indication of a southward recession of the isotherms in the Permian, and a renewed northward advance in the Lower Triassic. This continued until the middle of the Jurassic, but the farthest north was never again reached in the Pacific waters.

In the Upper Jurassic and the Lower Cretaceous another considerable southward recession of the isotherms is indicated, followed by a renewed northward advance in the middle of the Cretaceous. But this advance did not reach so far north as that of the Middle Jurassic. The Eocene epoch shows the temperature of the west coast nearly holding its own, but with a probable slight reduction. The Miocene climate had grown considerably cooler than that of the Eocene, and by the Pliocene it was already rather cold as far south as California. The early Quaternary climate was probably even colder than the Pliocene, for there we have the local ice-sheets in the high mountains of California. The post-Glacial amelioration of climate is as distinct here as it was in eastern America, and in Europe, and probably as short-lived. Middle and late Quaternary time was probably much longer than we have been accustomed to consider it, and there have doubtless been considerable fluctuations in our climate in that period, but we have as yet been unable to decipher these in the geologic record of the west coast.

SOME TESTS OF ACADEMIC EFFICIENCY[1]

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I HAVE come here from Boston for the simple purpose of manifesting the good will of an eastern institution to this vigorous university in the middle west. I need not remind you of the historical connection between Massachusetts and Kansas, but I should like to express the hope that frequent interchange of academic courtesies may at any rate keep alive the memories of that interesting connection. My mission here, however, is extremely simple and my duty entirely congenial. It is merely to congratulate you on this new exhibition of western energy and to join with you most heartily in the dedication of your splendid laboratories to the great purpose for which they were designed, the pursuit of science and its application to the problems of to-day.

I need not assure you that I have come here in no spirit of eastern superiority. In fact, if there is anything of east and west in my mind at all it is the old suggestion that the wise men of the east displayed their wisdom in going to the west for inspiration. I believe that this might well be done more frequently to-day.

But what impresses me most in a visit such as this is not so much the difference between the east and west, not so much the distinction as the points of similarity. The old distinctions seem to be rapidly disappearing and all are recognizing that the prosperity of one part of the country is intimately bound up with the prosperity of every other part. And there is no field of our national activity in which this is more clearly recognized than in the field of education. There have been differences, there have been jealousies, there have been rivalries between different colleges and technical schools. There are some of these differences and rivalries still left, but never before was there a time when the essential solidarity of the whole educational world was more clearly recognized, and when men saw so well as they do to-day that in all of our colleges, universities and technical schools we are fighting, if we are fighting at all, on the same side. Rivalries in some sense there must needs be, but no longer do we desire weak rivals. We want our rivals to be strong and we want them strong so that in the process of emulation and of competition we may be all forced to higher levels and there may be a general trend upwards rather than downwards.

[1] An address delivered at the dedication of the engineering laboratories of the University of Kansas, February 25, 1910.

Now it seems to me that in the process of striving to raise our standards we are a little apt to slavishly copy what other people are doing without clearly recognizing why we are copying them and what we are striving to attain. One college opens a new department in some sphere of activity; another thinks it is bound to do the same thing, although the local conditions may be totally different. If one school of engineering establishes a new course another is sure to follow with a similar course. We need a measuring rod to determine whether our level is above or below our competitors. How are we to reach a real standard of efficiency? How are we to know whether our institution is better or worse than some other institution? Of course various standards have been suggested. The great objection to most of them is that they are too mechanical. The best part of any educational institution is a spiritual thing and a spiritual thing must be spiritually discerned.

Now one of the institutions in this country which is doing its best to carry out a leveling process and trying to raise the institutions of the country is the Carnegie Foundation for the Advancement of Teaching. Its course is so brief that none here can have missed the opportunity of following it. Founded only a few years ago by Mr. Carnegie for the avowed purpose of pensioning professors who had long served their country as teachers and investigators, it is being put by those who have managed it to a quite different purpose and that purpose is to standardize our institutions.

I am not going to discuss what the foundation has done or is doing, but I should like to refer to a report, the advance sheets of which the Carnegie Foundation has just issued, under the title "A Comparison of Academic and Business Efficiency." The fundamental idea that suggested the drawing up of the report is one that must attract us all. It was to obtain a report on the efficiency of different educational institutions looked at from the view-point of a business man. To this end the foundation employed the services of an accomplished engineer, Mr. Cooke, and asked him to report on a number of educational institutions in this country. He was instructed to employ the same methods in his investigation that he would if he were reporting on the efficiency of a cotton mill or an automobile factory. To simplify the problem he was to confine his attention to eight institutions; to further simplify it he was to deal with a single department in each of these instituions; that department happened to be the department of physics. The report is a lengthy one—those of you who are interested will doubtless read it for yourselves—but I may just sketch with extreme brevity the fundamental guiding principle.

Mr. Cooke begins with the truism that if you are to test the efficiency of a factory from a business point of view you want to know the cost of the working of the machinery. He therefore proceeds to

discuss how much it costs to train men in physics in these different institutions and sets up a standard of measurement of what he calls the "student hour," the cost of teaching a student the subject of physics for a single hour. After an elaborate system of figures and a great deal of computation he discovers what is supposed to be the cost of teaching a student in physics for one hour in Harvard and Boston Tech, and these different institutions. Now whether his figures really represent the cost or not is questionable, but there can be no doubt that they do not gauge the efficiency of the institutions under consideration.

The efficiency of an automobile is not gauged by its cost, and certainly the efficiency of Harvard or Boston Tech is not gauged by their cost. You must of course look to the product. Now a man of Mr. Cooke's acumen could not overlook so obvious a fact, although he passes it over with almost unpardonable brevity in a report that professes to deal with the question of *efficiency*. He does not always seem quite true to himself. He tells us in one place that "the cost per student-hour has absolutely no value in distinguishing relative educational values." Elsewhere he says "certainly some idea of quality will be gained by simply knowing the cost." However, he does recognize that the *quality* of the product must be tested before we have any real gauge of efficiency, but when it comes to suggestions for a test of quality he formulates a plan that a serious educator could regard only with laughter or with tears. Here it is—let us establish a central bureau to which may be submitted the examination papers and the answers from the five highest and five lowest students, and let the central authority assign marks for the difficulty of the questions and the rigor with which they were answered. I shall not presume upon your patience by pointing out to what abuses such a plan would be exposed, nor how paltry a contribution it is towards the solution of an extremely important national problem. I should like, however, to call your attention to various matters to be kept in view when we set out on the task of testing the efficiency of any educational institution.

I would remark at the outset that the matter is extremely complex and that no wise man would even dream of giving a *numerical* measure of the efficiency of Harvard or the University of Kansas. He would no more do that than he would say that the efficiency of his friend Jones is 62, and of Smith is 55. On the face of it, such apparent accuracy is ridiculous. But we do want to know in a general way how we are to gauge efficiency, and I need only sketch the process which is a fairly obvious one. The natural way of attacking the problem would be to attack it directly. We are interested not in the machinery but in the product. The obvious procedure would be to examine the product in the different institutions and see how they stand relatively to one another. We would have, of course, to set out with some funda-

mental conception of what all of these educational institutions are striving for. Unless we agreed about that we could not possibly agree as to their efficiency. Fortunately, there is general agreement to-day that the aim of all educational institutions is a social one. The University of Kansas, and Boston Tech and Columbia University and all the rest are striving to this great end—to train men to serve the state intelligently, honestly and effectively. We are all attempting that. To what extent do we succeed relatively to one another?

Now, the natural process, I say, would be to examine the product of these different institutions and see whether men coming from these different institutions have "made good." This, however, is no easy matter where there are thousands of men to be considered and the gauging of the social efficiency of a single man is so difficult and delicate an operation. And then, you have to remember that the "making good" by an individual may have really little to do with the educational institution in which he has been trained. I had the honor of being brought up in the English university of Cambridge, which has been spoken of by a poet as a "nest of singing birds," for the reason that that university has produced, if I may use the term, an extraordinarily large number of great poets. But no one seriously suggests that the poetic power of Tennyson or Wordsworth had much to do with his training in the University of Cambridge. And so it is with the actual making good of a great many of our leading men; in most cases it is only indirectly due to the training they received in the university. Then you must bear in mind that an extremely important factor in the making of good flour is to have good grain, and that one institution might be as efficient as another, but yet for the lack of good grain not turn out so fine a product. Thus you would have to gauge not only the graduate, but the men at entrance, and this would greatly complicate the problem. Practically, then, I think, you would have to proceed indirectly by carefully examining the means that were employed in the institution to produce the results. If you bore in mind the idea of social service as a thing toward which we are all striving, you would have to begin, I suppose, with some estimate of the relative social value of a college education and the education in a professional school, taking each at its best. The aim of a college is to train a man broadly and so develop every side of his charcter that he can devote himself to the duties of citizenship in whatever special sphere of activity he can be most effective. The professional school does not neglect breadth of outlook or the duties of citizenship, but it bends its powers to the education of men for the service of society through the medium of definite professions. To gauge the relative value of these two schools you would need to decide whether it was more important to have an alert broad-minded man with no professional skill, or a man who could

set your leg if you broke it, or bridge the Mississippi if you wanted to cross it. It would be an extremely difficult question to decide, but you would have to do it somehow if you wanted to solve this problem numerically. Then if you confined your attention to professional schools you would need to estimate the relative value to the community of a doctor, lawyer, clergyman or engineer, and so on. In doing this you would necessarily take into account local needs and local peculiarities. You would have to consider, as a single sample of what I mean, whether there was a real demand in the community for an increased number of doctors, and so with the other professions.

Here to-day we are celebrating the foundation of buildings which are to be devoted to science and its applications, and so it would seem natural to consider that kind of educational effort somewhat more minutely. You would have to begin with deciding on the usefulness to the community of an education such as is being given in this institution, and in particular in this school of engineering where men are trained in the sciences for the service of the state. Now, it is such a commonplace to-day that science has revolutionized the world that I shall not weary you with attempting to demonstrate that fact. At the same time I should like to say in passing that, like many another commonplace, it is too often neglected in actual practise. It seems that individuals and states in making provisions for education constantly fail to recognize how enormously important to the welfare of the state it is that men should be trained in science, and in its application to every branch of practical life. We live in an age preeminently scientific, and if we are not able to cope with a problem scientifically we can not cope with it at all. But not only is a scientific training essential anywhere to any country to-day, it is, I think, peculiarly important in this country at this particular time. It seems to me that one of the great dangers of our democracy is the prevalence of the idea that one man is as good as another. It is an idea founded on an erroneous theory of democracy and one that appears utterly false from a scientific point of view. It too often gives support to the doctrine that any man will do for any position that he is clever enough to get. Nothing has surprised me more in moving about this country than to see countless instances of men who have had no adequate scientific training employed in the service of cities and of states, to do work that really needs a very considerable scientific equipment. They are amateurs doing the work of professionals. We have suffered too much at the hands of these amateurs, and we must remove them—root and branch. We must educate our communities in such a way that it will shock their moral sense to see a man, let us say, administering a department of public health who knows little or nothing of biology and bacteriology or any of the other fundamental sciences that enter into the very heart of his

work. Then we have to bear in mind that this nation is peculiarly given to extravagance. This is due largely to the optimism of the American people, a quality on which so much of America's success depends. But it has its drawbacks, like other good things, and the spirit of extravagance may yet drive us upon the rocks. We must not forget that conditions are rapidly changing and that what might suffice for a past generation will not do to-day. A generation ago we could speak of our natural resources as practically unlimited, now we begin to see their end—at least in some directions. And apart from this we must recognize that under any circumstances *waste* is a sin and that the record of progress is largely the record of the elimination of waste. We shall have to make up for the diminution of our natural resources by new applications of science which will make ten blades of grass grow where one grew before, and by new inventions which will save fifty per cent. or more of the waste in most of our industrial processes. However, even without any new inventions we could easily make enormous savings by the proper use of existing knowledge. Let me give you a single example. A few years ago a graduate of the Massachusetts Institute of Technology, trained in the department of biology, was appointed to an administrative post in one of the great cities. He invented nothing new, but merely joined common sense and executive ability to the scientific knowledge that his training at "Tech" had given him. Before long he had given the city a much better service than it had ever had before, and at the same time had *saved it more than a million dollars each year.* Suppose you multiply the million dollars thus saved by even a very small fraction of the thousands of men trained each year in the scientific institutions of this country and you may form some estimate of the saving grace of such institutions and of their value to the community.

I think, then, that there can be no question that you would have to put in a very large factor of usefulness, if you were estimating the value of such an educational institution as we are considering to-day—at least if you realize in any adequate degree the importance of scientific knowledge in public and private life. And, of course, a not unimportant element in such scientific knowledge would be a knowledge of physics, and under ideal circumstances this knowlege might be at least partially tested by Mr. Cooke's method, to which reference has already been made. It would, however, at best be only a partial test of knowledge, and it would neglect a great many factors of the first importance. May I remind you that *knowledge* is very far from being everything and that much of our educational work to-day and in the future must be to deliberately *smash up the idol of knowledge.* We are peculiarly prone to this form of idolatry in a scientific school, for science rightly lays a great stress on facts and their accurate apprehension. We are

very apt to overestimate the value of such knowledge, and it is because we have done this so much in the past that there has been so much disappointment in many quarters over the results of scientific teaching. It is a fact that water is composed of oxygen and hydrogen, and it is the knowledge of such facts that is tested by such examinations as Mr. Cooke proposes. But, except to a very few, such knowledge profits little or nothing—what is infinitely more valuable is an understanding of the method by which the facts are reached and an appreciation of the spirit that compels their investigation. Here, as elsewhere, it is the spirit that giveth life, and any test of efficiency that ignores the spirit and deals only with the bare fact is a mockery.

It would be a monstrous oversight to ignore the method and the spirit of the teaching. Are the pupils trained by a mere grind over knowledge, a mere hammering in of facts—enough perhaps to ensure that they reach the requisite 50 or 60 per cent. in Mr. Cooke's examination? We must all know schools that would appear to be highly efficient from such a test, and which are really extremely inefficient; and on the other hand some of our best institutions might not make a very good show when subjected to Mr. Cooke's scrutiny. At the Boston Tech a method has been in vogue for long that is there deemed highly satisfactory—it is known as the "do-it-yourself method." The students are put as much as possible upon their own resources and learning is not made easy where it seems better for a man to experience the apparent hardship of overcoming a difficulty for himself.

Then, when considering method, we should want to know whether the students are taught to master fundamental principles, or to spend most of their time over details or particular examples. Is it made manifest to them that the details of practise are constantly changing, that what is good in that respect to-day may be antiquated to-morrow, whereas fundamental principles, like the brook, go on forever?

As to the spirit of the teaching—is it possible to overlook the character of the teachers? Are they men who understand the depth and breadth of their calling? Do they take a large view of the life of to-day, and have some prevision of to-morrow? Are their circumstances such as to make this larger outlook possible or probable? Are they narrow specialists or broad-minded, far-seeing men? Are they paid so that a reasonably full life is a possibility, or are they so ground down by poverty that they must give most of their thought to the vexed question of the cost of living?

Finally, is a successful effort made by the teachers to convey their largeness of view and breadth of outlook to their pupils? Do the students learn to understand that science does not affect mankind merely on the material side? Do they see that all the changes that science has brought about necessarily involve a profound mental and

spiritual change—a change, so great, indeed, that it is well-nigh impossible for us thoroughly to sympathize with our grandfathers? Do they realize that science has thrust us into a new world and that our new surroundings have made us new men? Unless they appreciate this they can not be in real communion with the life of this age. They must live more or less apart, and move away from the great current that is sweeping the world along. Like Bernard Shaw, they must find that they were born in the seventeenth century and that they have not yet outlived it.

I might express this last test of efficiency otherwise by saying that you must look to the *cultural* element in the teaching of science—but I am afraid of the word "culture." It has been so terribly abused. Some speak as if the test of culture were the knowledge of Latin, or of Greek, or of French literature, or of Italian painting, or of what not. As a matter of fact it is none of these things, for I take it that the root of culture in any worthy sense of that word is *the possession of an ideal that is broad enough to form the basis of a sane criticism of life*. I hope that I need not turn aside to demonstrate the competency of science to present such an ideal. I willingly admit that some such ideal may be reached by various paths, through the study of literature, or of art, or of science. I should be the last to suggest that these are rival or mutually exclusive pursuits or that any one can justly claim a monopoly of culture. To know the best that has been said in literature and to use this as a touchstone in the criticism of the life of to-day, or to reach through art the ideal of perfection in form and color and make this broad enough to embrace life as a whole—each opens a promising avenue to culture. But how can a criticism of life be broadly enough based to-day unless the main results of scientific investigation lie at its roots and the method and the spirit of science be in the atmosphere that surrounds it? It can not, I think, be broad enough, unless we greatly exaggerate the part that science has played and is playing in the modern world. And I do not think that we exaggerate it, for practically all must recognize that there are few important problems of life to-day that science does not touch and touch most closely. This being the case, can a school be declared efficient that fails to give its students a vision and a grasp of the scientific ideal—an ideal that will guide them in the solution of all the complex problems that face individuals and face the state?

THE PROPHECY OF FRANCIS BACON (1560–1910)

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I. *Bacon and the Spirit of Discovery.*—There are several ways in which the importance of a philosopher may be estimated. He may be regarded as an exponent of his times; that is, as a representation in which the manifold tendencies of an age are focalized and idealized. Or he may be regarded as the author of a panorama of existence, of a world-view or system, which, while it may be superseded, will always retain enough of logical and imaginative coherence to make it typical and classic. Or the philosopher, like other servants of mankind, may be judged according to the degree in which he has been confirmed by posterity. Judged by this last standard, the great philosopher will be the philosopher who, while he may, like Bacon, have been born three hundred and fifty years ago, is nevertheless modern, in the sense that he is identified with important ideas which are now generally held to be true. This brief summary aims to present the Bacon that is living to-day in our common opinion, in our expert knowledge, and in our dominant ideals.

Any one who considers Bacon in relation to European civilization of the modern period must be impressed with the degree to which he represents its progressive ideas. Those characteristics of the sixteenth and seventeenth centuries which are most marked in Bacon are the characteristics in which they anticipate later centuries. It is possible for our immediate purposes to reduce these characteristics to one: the disposition, namely, to look for a betterment of human life from the advancement of knowledge. "Advancement of knowledge" does not here mean the education of the individual, but the winning of new truths by the race and for the good of the race. We may call this the spirit of discovery, where "discovery" is used both in the theoretical and in the practical sense. Bacon himself was not a discoverer of new scientific truths, but the discoverer of the art of discovery. As he expressed it, he "rang the bell that called the other wits together." While it is doubtless inaccurate to attribute so general an idea to any individual authorship, Bacon was its greatest prophet. His brilliant literary gifts, his imagination, his sanguine temperament, his breadth of view and his native regard for utility, the very qualities that helped to unfit him for exact research, made him the most important medium through which the idea of discovery, or of intellectual conquest, has gradually become the hope of mankind.

II. *The Baconian Reform.*—This idea was defined by Bacon largely in opposition to what he believed to be the blindness and errors of his own and earlier times. Philosophical literature nowhere else contains so acute and so comprehensive an examination of man's intellectual bad habits. Bacon's criticisms may conveniently be brought together under four heads.

First, he defined the persistent error of *anthropomorphism.* It is customary for man to fashion things after himself. He is deceived by what Bacon calls the "idols of the tribe" or the prejudices characteristic of human nature in general, and by the "idols of the den" or the prejudices peculiar to the individual. But if he is to view nature as it is, he must efface himself.

Second, he found the thought of his own time to suffer peculiarly from *conventionality.* It was customary for men to accept what was current and supported by general opinion. There are two important means through which arbitrary or ungrounded ideas are foisted upon belief: language, which gives rise to what Bacon calls the "idols of the market-place," and established systems, or theories which have the stage, and which give rise to what Bacon calls the "idols of the theatre." In the interests of truth it is necessary to guard against the suggestive power of words, which are often obscure or even meaningless, and against the inertia of doctrines that have acquired repute and prestige.

Third, it was customary in Bacon's time, to a degree that is scarcely intelligible to-day, to assent to theories of nature on grounds of *authority,* ecclesiastical or political. Bacon is among the first to formulate the principle of tolerance, according to which there is hope of knowledge, provided only that the mind be free from external constraint. The truth-seeking mind can acknowledge no obligations except to evidence.

Fourth, Bacon attacked the tendency, common at the time of the Renaissance, to rely on *antiquity.* The essentially modern character of Bacon's mind is nowhere more apparent than in his repudiation of the idea that dominated the revival of letters. He detected the dangerous fallacy which had arisen with the new study of the ancient languages and literatures. Historical retrospect inverts the intellectual values of the race. The wisdom of the ancients is but the folly of youth—*Antiquitas sæculi juventus mundi.* The hope of knowledge lies not in a return to childhood, but in a maturity yet to come.

III. *The Baconian Survey.*—As a pioneer in a new intellectual enterprise, it fell to Bacon to draw a rude map of the settled domain and border wilderness of knowledge. It is impossible here to enter into the merits and demerits of his classification of the sciences. Most interesting to us of the present is his explicit provision for what is

now known as "applied science." But there can be no doubt of the service which Bacon rendered in making such a classification at all. To Bacon modern science is largely indebted for the sense of solidarity that obtains among all special investigators. He was, in a measure at least, responsible for the organization of the Royal Society in London, and of similar societies on the continent. He inspired the collective scientific movement of the Encyclopædists; and, directly or indirectly, the systematization of science made by Comte, Spencer and others. The present idea, then, that the several sciences are the members of one body, and that those who serve them are serving in one army to achieve the conquest of the unknown, is an idea to which Bacon testified clearly and effectually.

IV. *The Baconian Method.*—But Bacon did not merely point out the promised land and exhort men to discovery; he organized a plan of campaign. There is an opinion to the effect that while Bacon was enlightened in his general ideas, he was benighted in his particular ideas. This opinion is entirely unjust. Bacon does make many of the mistakes current in his time; and he deliberately makes many loose statements in the hope that they may prove suggestive and stimulating. Furthermore, he necessarily uses terms, such as "form," which, because they were borrowed from Greek and medieval thought, suggest to our minds something pre-scientific and obsolete. But this very term, as actually employed by Bacon, is the closest approximation in his time to the modern conception of cause, as employed in such sciences as molecular physics and chemistry. Furthermore, and be it said to his great and enduring credit, he was the great systematizer and popularizer of experimental method. The incompleteness of the Baconian method is the incompleteness of the experimental method. Although he did not by any means ignore it, it is true that Bacon did not adequately realize the importance of the quantitative or mathematical formulation of scientific laws. But this fact in no wise affects the correctness of his statement of the experimental method. The Baconian plan of research, avoiding technicalities, may be said to contain four important ideas, all of which have been approved and employed in subsequent scientific procedure.

His first and fundamental idea is that of *observation.* Bacon never wearies of reminding us that the mind must be brought into direct contact with things. In the study of nature, we may see, he believes, by the "ray direct." To avoid verbalism, dogmatism or ambiguity, it is necessary that the mind should be open to the facts, and that it should follow their leading. "We can only conquer nature by first obeying her." But Bacon understood the fruitlessness of desultory observation. For purposes of explanation all facts are not equally significant.

Hence, secondly, he was led to define certain methods or *canons of induction.* It was Bacon who first called attention to the importance of "glaring" or "striking" instances, in which the phenomenon under investigation is thrown into relief; "parallel" instances, which permit of the argument from analogy; and "crucial" instances, which serve as tests of contrary hypotheses. From Bacon, Mill derived the methods to which he gave such prominence in his *Logic,* the methods, namely, of "agreement," "difference" and "concomitant variation." By means of these methods it is possible to single out from among the circumstances attending or preceding the phenomenon to be explained, that which is its probable cause. That which is present when the phenomenon is present, which is absent when the phenomenon is absent, and which shows like quantitative changes, may be assumed to be connected with the phenomenon, and to point the way to its explanation.

But, thirdly, it is necessary to supplement observation of the natural course of events with artificial *experiments.* Nature, like men, will reveal her secrets only when put to the torture. Bacon was a consistent advocate of the first-hand manipulation of natural bodies. He saw this to be the only method of study which afforded any prospect of laying bare the more "subtle" physical phenomena, such as heat, light and the transmutation of substances. The later development of physics and chemistry not only confirmed this judgment, but in several signal cases fulfilled definite predictions which Bacon based on it.

Fourthly, Bacon recommended the *comparative* and *historical* method. He was one of the first to appreciate the importance of studying all phenomena that develop, in different stages of their development. In the particular case of anatomy, he called attention to the importance of studying the structure of organs in their simpler forms, and using the results as a key to the complex forms.

V. *The Baconian Pragmatism.*—Bacon's extraordinary modernism appears not only in his definition of sound and fruitful methods of scientific study, but also in his conception of the relation of science to civilization. And in nothing is he so modern as in this. He asserted that the hope of man lay in his advancing knowledge and control of nature. This idea is undoubtedly a present commonplace, but there are few philosophers that anticipate the commonplaces of mankind by three centuries and a half! But the idea is too fundamental properly to be called a commonplace. It is the most fruitful idea in modern life, the main presupposition of progress. Bacon sought to promote learning for the sake of power. That this is essentially a modern idea will be apparent to any one who will study the motives underlying earlier periods of European civilization. The ancient world had its critical and its dogmatic idea of progress. The

former was that of national or racial aggrandizement, the conquest of territory and political control. The latter, contributed by the genius of Greece, was the humanistic idea of the intensive cultivation and refinement of human nature. These ancient ideas were superseded by Christian supernaturalism, which referred man's hope of salvation to another world which might be won by the repudiation of this. As christian Europe became secularized there developed the theocratic idea of a fixed system in which all human activities should be limited and controlled by religious authority. Finally, as a reaction against the established order, there appeared the idea of the Renaissance, an enthusiasm for antiquity, and desire to reverse the course of history. The modern idea, though it borrows something from all of these ideas, is fundamentally different. It bespeaks a solidarity of mankind in the enterprise of life, and in this manifests its christianity; and it derives from paganism a respect for human capacities, and a confidence in man's power to win the good for himself. But these motives are so united in the modern spirit as to produce something genuinely new. The good is to be won by the race and for the race; it lies in the future, and can result only from prolonged and collective endeavor; and the power to achieve it lies in the progressive knowledge and control of nature. This is the Baconian idea. The incentive to knowledge lies in its application to life. "For fruits and inventions are, as it were, sponsors and sureties for the truth of philosophies." Therefore, Bacon would have men of learning begin and end their study with the facts of their present environment. "For our road does not lie on a level, but ascends and descends, first ascending to axioms, then descending to works." In the last part of the *New Atlantis* there is a remarkable description of the riches of Solomon's House, the great museum and laboratory, the treasure house and workshop, which was "the lantern of this kingdom." The words with which the father of Solomon's House receives his visitors are a terse and eloquent summary of that which Francis Bacon prophesied, and which posterity has steadily achieved. "The end of our foundation is the knowledge of causes, and secret motions of things; and the enlarging of the bounds of human empire, to the effecting of all things possible."

JOHN DALTON AND HIS ACHIEVEMENT: A GLIMPSE ACROSS A CENTURY[1]

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IT is a melancholy reflection that the treasure laid up by great men in our memories should be corrupted often by the moth and rust of error. But, after all, this mischance roots in the nature of the case. Necessarily, our views of the past are synoptic, because the daily details, even of big events, escape us, much more the complex, ceaseless pulsations of the persons who have served their time and place rarely. Be we appreciative or critical, we lie under sore temptation to forget the inevitable limitations of human lot, and thus to lose perspective. Accordingly, my scientific colleagues,[2] with whom you have done me the honor to associate me in our effort to pay worthy homage to the genius of John Dalton (1766–1844), whose "A New System of Chemical Philosophy," although not completed in the second part of 1810,[3] had reached all its epochmaking significance, have requested me to introduce the subject with some account of the difficulties, amazing to us in our conditions, under which this strenuous pioneer labored. To this end, we must try to pierce the cultural inwardness of English life at the close of the eighteenth century, keeping in mind the peculiar qualities that characterize English science even yet.

I

As usual, the bare facts of Dalton's story need interpretation, the invisible atmosphere, their setting, imports much. Born 1766, in a little village of Cumberland, a county still remarkable for its sparse population, of a Quaker family, who eked out a precarious livelihood upon the home industry of woolen-weaving, Dalton's social relations isolated him from the chief cultural organs of the national life. Till the tender age of twelve he received such instruction as the local Friends' school afforded, and he appears to have made excellent use of his opportunities: then he went to work as a teacher there, and as a

[1] Read before the Research Club of the University of Michigan at its annual memorial meeting, devoted in 1810 to a celebration of the centennial of Dalton's atomic theory.

[2] Professor S. Lawrence Bigelow, of the department of chemistry, and Professor Karl E. Guthe, of the department of physics.

[3] It was never completed; the second part of the second volume did not appear.

hand in the paternal fields, for three years. At fifteen he migrated—literally walked!—to Kendal, forty-five miles away, where he taught in a mixed school, the venture of a cousin; and, remember, a mixed, local school in the England of that generation portends not a little respecting absence of amenity, appliances and opportunity. Here he spent twelve years, fruitful in many respects. For, the day's darg done, he contrived to improve himself by private study of Latin, Greek, French, mathematics, and "natural philosophy," with most important help and encouragement from John Gough[4] (1757–1825), the blind naturalist, celebrated by Wordsworth in "The Excursion."

> Methinks I see him how his eyeballs roll'd
> Beneath his ample brow, in darkness pained
> But each instinct with spirit, and the frame
> Of the whole countenance alive with thought,
> Fancy, and understanding; whilst the voice
> Discoursed of natural or moral truth,
> With eloquence and such authentic power,
> That in his presence humbler knowledge stood
> Abashed, and tender pity overawed.

In 1793 he removed to Manchester where, on Gough's recommendation, he had been appointed science tutor in New College, a Presbyterian institution, and, therefore, once more without the pale of national higher education; he held this position for six years, at a salary of $400. On the transference of the college to York, he resigned, and gave himself to private tuition, an exiguous vocation, sufficient for daily bread. But the Manchester experience proved a turning point, for it offered an environment wherein he could make pure science his avocation. From 1786 Dalton had been engaged in meteorological observations, and published his maiden work in the autumn of 1793—"Meteorological Observations and Essays." Printed for the author, it failed of due publicity. Thanks to his connection with the Manchester Literary and Philosophical Society, he read his famous paper, "Extraordinary Facts Relating to the Vision of Colours," in October, 1794, a month after his election. In 1801 he presented his first classical research, "On the Constitution of Mixed Gases," which was followed by three memorable papers, "On the Force of Steam or Vapor from Water and other Liquids in Different Temperatures, both in a Toricellian Vacuum and in Air," "On Evaporation" and "On the Expansion of Gases by Heat." In the last he enunciated the law of expansion of gases formulated by Gay-Lussac a few months later.

It was in 1802, after six years of research in chemistry, that he referred to the possibility of multiple proportionate combinations of the elements, in a paper entitled "On the Proportion of the Several Gases or Elastic Fluids Constituting the Atmosphere." The atomic symbols

[4] See "Dictionary of National Biography," sub voce.

devised by him are first found in his note-book under the date September 6, 1803; and, under the same date there is a table of atomic weights, showing that, by this time, he had grappled with the fundamental problem—that of fixed "relative weights of the ultimate particles of bodies." For unknown reasons Dalton appended it, in a somewhat different form, to a paper "On the Absorption of Gases by Water," read before the Manchester Society in October, 1803, but not published till November, 1805. The table was added during the interval between presentation and publication. The summer of 1804, as Dalton himself tells us, was the crucial period of the investigation. The first part of the first volume of the "New System of Chemical Philosophy," published in 1808, gives the mature theory, while the second part of 1810 describes the chemical elements in detail. Dalton was now forty-four. And it is significant that, although he had lectured twice at the London Royal Institution, and in Glasgow and Edinburgh as well, the French Academy of Science recognized his merits six years[5] before any native body. In 1822, Dalton being fifty-six, the Royal Society honored itself by his election. Another decade elapsed ere Oxford conferred her D.C.L., on the occasion of the second meeting of the British Association, and he was sixty-eight when Edinburgh enrolled him among her honorary doctors. In 1833, the government took note of his services, and he received a civil list pension, increased afterwards in 1836, when the announcement was publicly made under dramatic circumstances by Sedgwick, at the Cambridge meeting of the British Association. "The imagination may picture, if it can," writes Roscoe, "the feelings of the son of the poor Eaglesfield handloom weaver as he sat in the Senate House of the University of Cambridge listening to this eulogium—the observed of all observers."[6] As Sedgwick remarked in his striking speech, "without any powerful apparatus for making philosophical experiments—with an apparatus, indeed, many of them might think almost contemptible—and with very limited external means for employing his great natural powers, he had gone straight forward in his distinguished course, and obtained for himself, in those branches of knowledge which he had cultivated, a name not perhaps equaled by that of any other living philosopher of the world."[7] Evidently, then, Dalton wrought under grave disadvantages. What were they?

We would all agree, I take it, that certain results of human activity must remain intimately personal, and that, as a consequence, they must vary from age to age, or diverge even among different peoples in the same epoch. Art and poetry, religion and, possibly, some portions of philosophy, can not well escape these very subtle contrasts. But, with

[5] Cf. "John Dalton and the Rise of Modern Chemistry," Sir Henry E. Roscoe, p. 175.

[6] *Ibid.*, pp. 204, 205.

[7] *Ibid.*, p. 203.

Wissenschaft, particularly in that development of it known to us as positive science, the case stands far otherwise. Yet, even here, the unification of knowledge, each nation contributing its quota to the common fund, happens to be perhaps *the* achievement of the nineteenth century. Organization by countries, especially in the case of France, there was between the Renascence and the French Revolution; but a world-wide pact, embracing all effort, no matter where, did not eventuate then. Now, it is of prime moment for the present subject that the instruments of this recent unification have been the German university system, and the academies and institutes of France. By contrast, the English-speaking world possessed no such developed organs, if we except the Scottish universities where, naturally enough, Dalton met immediate recognition. Thus English science till but yesterday—*teste* even Darwin—has betrayed individualistic tendencies. These were never more evident than in Dalton's career, and during his life, moreover.

At the beginning of the nineteenth century, when Cuvier, in his "Rapport" of 1808, is extolling—and justly—the preeminence of France in the exact sciences, an extraordinary contrast manifests itself across the Channel. In the same year, John Playfair bewails the "incontrovertible proofs of the inferiority of the English mathematicians," and refers to "the public institutions of England" as its cause.[8] Eight years later, in a damnatory notice of Dealtry's "Principles of Fluxions," another writer notes it for a paradox that, Newton dead, his country "should, for the last seventy or eighty years, have been inferior to so many of its neighbours."[9] Once more, in the same review for 1822, a third critic deplores the state of affairs at Cambridge, where "for want of facilities" men "are apt to lose the spirit of investigation." Brewster's article in the *Quarterly Review*[10] which, as is well known, led to the foundation of the British Association, is no less sarcastic and outspoken. These attacks were directed against the English universities:[11] that of Babbage, the peg on which Brewster hung his exordium, had the Royal Society for its mark.[12] Now the extraordi-

[8] Cf. *Edinburgh Review,* No. XXII., January, 1808, pp. 249 f. A review of La Place's "Traité de Mécanique Céleste."

[9] *Ibid.,* No. LIII., September, 1816, pp. 87 f. (Dealtry was a fellow of Trinity College, Cambridge, and a fellow of the Royal Society.) Dalton himself made the same complaint in his lectures at the London Royal Institution (1810); cf. "A New View of the Origin of Dalton's Atomic Theory," Roscoe and Harden, p. 105.

[10] Vol. XLIII. (1830), pp. 305 f.

[11] Analogous circumstances produce analogous protests even now—*e. g.,* "Oxford at the Cross Roads," Professor Percy Gardner (1905).

[12] "Reflections on the Decline of Science in England, and on some of its Causes," by Charles Babbage, Lucasian professor of mathematics in the University of Cambridge (1830).

nary thing is that, prior to and during these years, or, to be quite exact, between 1774 and 1828, Britain had contributed at least a dozen discoveries of the first magnitude, and as many more of scarcely less importance. As you all know for what each stands, I need only mention Priestley, Black, Landen, Davy, Benjamin Thompson, Cavendish, Herschel, Nicholson and Carlisle, Dalton, Young, Wollaston, Ivory, Robert Brown, Charles Bell, Brewster, William Smith, Prout, Faraday, George Green and Rowan Hamilton. Still more wonderful, continental leaders were well aware of these contributions, and wont to emphasize them. In 1821, Cuvier gave most generous testimony: and Moll, of Utrecht, repelled Babbage's criticisms with no uncertain sound, remarking, "all must allow that it is an extraordinary circumstance for English character to be attacked by natives and defended by foreigners."

Although I can not comment upon the ramifications to-night, the puzzle has some obvious causes. The English universities were not scientific organs, but groups of residential colleges. The advancement of science was no *primary* part of their purpose, precisely as the laborious elevation of incompetents to a bare level of possible passability was no *primary* part of the purpose of the German universities or the French institutes. The colleges cherished their individuality fondly, because they aimed to produce a certain type of man for life—to anneal him by forming his *ethos,* and to fit him for the exercise of civic influence by giving him a respectable general acquaintance with the "things of the mind." In a word, the English universities did not exist to promote science or learning, any more than the continental organizations existed to provide an educational top-dressing for the sons and daughters of "the people." So, too, of pure thought. The apostolic succession of English philosophers—Bacon, Hobbes, Locke, Berkeley, Hume, the Mills, Spencer, even our contemporaries, Hodgson, Balfour, Shand, Haldane and Bertrand Russell—do not adorn the universities. Again, the peculiar position of the metropolis, its new university in the melting-pot at this moment, must be taken into account. Lacking the academic center, its scientific societies could not be organized for the advancement of discovery after the style of French and German associations.[13] These causes, together with the distinctive arrangement of English society a century ago, tended to render the great scientific pioneers lonely figures, sitting loose to the main expressions and modes of national culture. The wails over the condition of English science are traceable as much to this severance, with its absence of constant intercourse and cooperation, as to aught else. How Priestley and Dalton and Joule, Young and Davy and Faraday were hampered by these

[13] For example, in the preface to "A New System," Dalton makes the (to us) astounding statement, that he did not know whether the abstracts of his lectures, left by him for this express purpose, had been published in the *Journals* of the Royal Institution. Some five years had elapsed since their delivery!

circumstances is notorious. Others, like George Green, received no recognition whatsoever. In addition, the English passion for independence played its part. The demand for complete freedom, if it fostered the eccentricity of which the docile, drilled Germans complained, although it led to pig-headedness, as in Dalton's case, also proved greatly favorable to original genius. For, it is well to recall that more original notions, basal to modern science, have come from England than from any other land, even if, as with Newton and Darwin, France was to systematize *Newtonianisme,* Germany *Darwinismus.* England possessed no trained regiments to accomplish these things. Accordingly, if we remember all this, some apparent mysteries that cloak Dalton's career and mental characteristics begin to dissipate. In short, the Dalton we commemorate would have been nigh inconceivable had he been "born to the intellectual purple of the ancient universities"; but the Dalton we regret, who remained obdurate to Gay-Lussac despite Berzelius's intercession, might never have been. The qualities of the man, like his defects, pertained to his strong, wayward and undisciplined, if narrow and often uncouth, provincialism. *Qui a nuce nucleum esse vult, frangat nucem.*

II

Dalton maintained silence from 1793 till 1799, hindered, perhaps, by college duties. On reappearance, he soon dropped the rôle of meteorologist for that of chemist and physicist. The new line was taken in the paper entitled "Experiments and Observations on the Power of Fluids to Conduct Heat, with Reference to Count Rumford's Seventh Essay on the same Subject," read before the Manchester Society on April 12, 1799. The simple nature of his apparatus may be illustrated aptly from this communication.

> **Took an ale glass of a conical figure, 2½ inches in diameter, and 3 inches deep; filled it with water that had been standing in the room, and consequently of the temperature of the air nearly. Put the bulb of a thermometer in the bottom of the glass, the scale being out of the water; then having marked the temperature, I put the red-hot tip of a poker half an inch deep in the water, holding it there steadily for half a minute; and as soon as it was withdrawn, I dipped the bulb of a sensitive thermometer about ¼ inch, when it rose in a few seconds to 180°.**[14]

Then follow the tabulated temperature results. Another experiment, described in the same paper, suffices to show that Dalton had pondered the discontinuity of matter thus early. Having mixed hot and cold water for half a minute, he proceeded to determine whether the upper layer became warmer than the lower. Observing that it did not, he remarked: "If the particles of water during the agitation had not

[14] *Memoirs of the Literary and Philosophical Society of Manchester,* Vol. V., p. 381.

actually communicated their heat, the hot ones ought to have risen to the top, and the cold ones subsided, so as to have made a material difference in the temperature."[15] Furthermore, these and many other experiments afford us indications of his mental habit as a scientific investigator. Conceptual processes find him at his best; his theoretical expectations and deductions are good. In experiment he is not so happy, and what we understand by "fine" or refined work occurs seldom. Thus, in the case just cited, Dalton infers "that the expansion of water is the same both above and below the point of maximum density." But, when he comes to determine this crucial point precisely, he goes wide of the mark, setting it at 36°.

These references may enable us to grasp his manner of approach to a problem, and to realize his general plan of attack upon the atomic constitution of matter as it stood when he entered the field.

I wish that space permitted me to present some consecutive account of the doctrine of "matter" as it developed down the ages—but this is impossible. The subject deserves attention, because so bemused in the minds of the laity. And not only this. Scientific men themselves misconceive it at times, not deliberately indeed, but because, absorbed in researches of immediate moment, they have not troubled to follow the marvelous story with patience. The long, tortuous endeavors that culminated in Dalton's atomic theory, with its kernel, the law of multiple ratios, are the tale of man's attempt to reduce his notion of "matter" to *conceptual* simplicity; this to the end that it might be rendered an obedient instrument. Freed from contingent accessories, the central problem was this: Given such a vast multiplicity and variety of phenomena as the "substantial" world presents, how can all be grasped under a single, synthetic idea? Plainly, whenever man began to reflect upon nature, he encountered this sphinx. The elusive, yet persistent, relationship between the one and the many forms part of ancient history in science no less than in metaphysics.

Now, stating the situation very synoptically, and omitting the metaphysical reference in favor of the natural-scientific, it may be affirmed that the problem itself is also a many in a one. For, if we are to reach clear concepts about natural phenomena, we must reckon with three investigations at least. In the first place, a particular phenomenon must be selected, and treated as the starting point. This done, it is requisite to obtain an all-round view of *what* it is. In the second place, one must proceed to elucidate its relations to other phenomena, preferably to those which evince evident, or apparent, kinship. In the third place, order must be induced in the relations that have thus come under observation by reducing them, as far as possible, to numerical expression. The primary methods of weighing, measuring and enumeration

[15] *Ibid.*, p. 385.

must be invoked. This achieved, we may assert that we have arrived at that species of conceptual simplicity which we call a "law of nature." On a broad view, it is fair to say that, prior to Dalton, investigation and fancy pursued the one (*i. e.*, the conception of "matter") through the many (*i. e.*, these three aspects of the problem). For, on the whole, till we come to J. J. Becher (1635–82) and G. E. Stahl (1660–1734), the element theory held the field. And this is only to affirm that men were trying to master the properties of particular bodies, while reserving the remoter question of the ultimate constitution of "matter." Roger Bacon's view, probably the least fantastic we possess, is exceedingly significant of this.

> There are four Elements—fire, water, air, earth; that is, the properties of their condition are four—heat, coldness, dryness and wetness; and hyle is the thing in which there is nor heat, nor coldness, nor dryness, nor wetness, and a body is not. And the Elements are made of hyle; and each of the elements is transmuted into the nature of the other element and everything into everything else. For barley is a horse by virtual possibility, that is, occult nature; and wheat is a man by virtual possibility, and a man is wheat by virtual possibility.

The age of phlogiston, with its theories of combustion, marks a move to the second question. Men are now engaged in an effort to relate phenomena. Or, as Stahl puts it, in his conspectus: Combustible substance *minus* phlogiston is burnt substance—*e. g.*, metals, sulphur, phosphorus, etc., *minus* phlogiston, are metal calxes, sulphuric and phosphoric acids, etc. On the other hand, burnt substance *plus* phlogiston is combustible substance—*e. g.*, metal calxes, sulphuric and phosphoric acids, *plus* phlogiston supplied by carbon, are metals, sulphur, phosphorus, etc. In a word, the most different phenomena, such as the burning of carbon and the calcination of a metal, are shown to belong to the same class, and to be explicable by a simple conceptual hypothesis. Finally, when Lavoisier sent phlogiston by the board, the third question came to the fore, and men began to ask, How can we weigh, measure and enumerate the exact degree of relationship between the properties of substances? Dalton ranks among the great epoch-makers, because he first brought this inquiry within the range of practicable *uniformity*.

Discussions about prior discovery, over which much time and no little temper have been expended, prove profitless affairs, as a rule. You see, error and loyalty are human. For instance, I am well aware that scientific chemistry is dated usually from 1776, when Lavoisier made the balance *the* chemical instrument: but you will bear with Sadler[16] and me if we travel a little farther back and, as loyal sons of alma mater, find the initial point in the classical investigation of latent heat, conducted by Black between 1759 and 1763, at Glasgow. Nevertheless, as Dalton's priority has been impugned, we are bound to consider the facts.

[16] Herbert C. Sadler, professor of naval architecture.

Of course, every one knows that the conception of the discontinuity of "matter" appears in ancient history. And, when we descend to modern times, Boyle (1627–91) speaks of corpuscles, Boerhaave (1668–1738), Albrecht von Haller's master, of *massulæ*. Moreover, Dalton was a youth of only seventeen when the most important developments occurred. First, and with special reference to the framework of possible method, we have Lavoisier's (1743–94) celebrated memoir, "Reflections concerning Phlogiston," where he dismisses the dominant theory in sarcastic terms,[17] and establishes the *quantitative* method on a firm basis. In the same year (1783) Bergman (1735–84), the last of the great phlogistic chemists, published his notable work on what he called "elective attraction" (*i. e.*, affinity), a phenomenon attributed by him to the attraction between the most minute particles. Naturally, Bergman's table of "single elective attractions in the moist way, and in the dry way," with its curious alchemical signs, was a description of *qualitative* relations. It marked the beginning of investigation of mass action, and provoked the striking researches of Berthollet (1748–1822), who, in 1799, presented his paper, "Recherches sur les lois de l'affinité," out of which grew his major work, "Essai de statique chimique" (1803). The main result of his assault upon Bergman was to show that chemical change depends, not merely upon the affinities of the substances involved, but upon their *quantities*. In other words, a new method asserted itself. For, as Berthollet says:

> To find the affinity of two substances towards a third, in accordance with the conception we have now gained of affinity, can mean nothing other than to determine the *ratio* in which this third substance divides itself between the two first.

Therefore, chemical change hinges upon the nature of the relative *masses* of the substances involved, but, "to determine the ratio of the affinities of two substances towards a third . . . is attended by unsurmountable obstacles." Here was the blank wall, so to speak, that shadowed Berthollet's services till the time of Guldberg and Waage (1864). As Berthollet stood to Bergman, so did Proust (1755–1826) to Berthollet. Baffled in every attempt to determine the distribution of salts in solution, Berthollet had good reason to doubt the doctrine of constant composition. Here was Proust's opportunity. Having distinguished between "combinations of elements" and "associations of combinations," the latter variable under analysis, Proust was able to enunciate the law of fixed proportions—in his own words, "Election and proportion [*i. e.*, affinity and fixity of composition] are the two poles about which revolves immutably the whole system of true compounds, whether produced by Nature or by Man"; or, as Lothar Meyer phrases it, "Definite chemical compounds always contain their con-

[17] Cf. "Œuvres," Vol. II., pp. 623 f.

stituents in *fixed* and *invariable* proportions." Notice, in the words I have italicized, the unanimous trend towards *quantitative* measurements and accuracy, the ruling notion being that of *numerical ratio.*

We come to closer quarters with our central theme in the work of Richter (1762–1807), an investigator, it is important to note, obsessed by mathematical methods. Despite his obvious idiosyncrasy, Richter arrived at the law of equivalent ratios—"The qualities of acids and bases equivalent in one neutralization are equivalent in all." In 1802 Fischer made Richter's conclusions known to Berthollet, and chemical ratios became an integral part of the science. As Wollaston says, in 1814:

> It is to Richter we are originally indebted for the possibility of representing the proportions in which the different substances unite with each other in such terms that the *same substance* shall always be represented by the *same number.* He discovered the law of permanent proportions.[18]

The experimental proof was clinched by Berzelius in 1811–12, and the law of "permanent" or "definite" ratios, as it is called now, put the problem of composition on a practicable footing.[19] It should be noted also that, in stating the numerical values of the elements, Dalton employed some determinations of other chemists, at all events as checks.

We are now in a position to see that series of complicated researches, all looking to *quantitative* results, furnished Dalton with material which enabled him to render the atomic theory perspicuous and applicable from the very outset. Notwithstanding, to him must be given sole credit for the final simplification, which had been exercising his mind for some eighteen years—since 1790, in fact. A quotation from Berthollet's "Essai" (1803) may suffice to emphasize the long step due to Dalton's insight.

> Some chemists, influenced by having found determinate proportions in several combinations, have frequently considered it as a general law that combinations should be formed in invariable proportions; so that, according to them, when a neutral salt acquires an excess of acid or alkali, the homogeneous substance resulting from it is a solution of the neutral salt in a portion of the free acid or alkali. This is a hypothesis which has no foundation, but a distinction between solution and combination.[20]

Undoubtedly, events tended towards the new climate of opinion, nay, this had become so far prevalent that the Irishman, William Higgins (17?–1825) came nigh playing Wallace to Dalton's Darwin. Indeed, in 1814, he raised a claim to priority, which was disproved at once by Thomson, the Glasgow chemist who had made Dalton known. This Higgins is to be distinguished from his uncle, Bryan Higgins

[18] The italics are mine.

[19] Reference should be made to the classical experiments in further confirmation by Stas (1865).

[20] Cf. Lambert's English translation, p. 39.

(1737–1820), who, in 1775, in a prospectus of lectures, proposed to discourse of "his notions and experiments concerning the primary elements and properties of matter," and of "experiments, observations and arguments, persuading that each primary element consists of atoms homogeneal: that these atoms are impenetrable, immutable in figure, inconvertible, and that, in the ordinary course of nature, they are not annihilated, nor newly created." He also conceived of atoms, of simple particles, and even of gases, as uniting *sometimes,* in approximately, if not completely, fixed proportions. Yet, he never arrived at true causes, because his experiments failed to dove-tail with his advanced theoretical suggestions. Accordingly, the explicit variety of the former destroyed the implicit unity of the latter, and the *status quo ante* was maintained.[21] William Higgins, the claimant of 1814, published his book[22] in 1789. It contains forecasts of the atomic theory, such as the following:

> I am likewise of opinion that every primary particle of phlogisticated air is united to two of dephlogisticated air, and that these molecules are surrounded with one common atmosphere of fire.[23]

But, after all, less than a dozen pages of the 300 deal with the subject; and, although he assigned causes for definite proportion and saturation in a few cases, he never suspected a simple, universal and necessary law. His real acuteness led him to see that combining particles had the same weight (multiple proportions), but he missed his chance to generalize in a maze of suspicions directed against the phlogistic theory, which had already lost its primacy; his indolence also hindered him, like his eccentricity.

III

Finally, coming to Dalton's characteristics as a thinker, we may find the clue in his forcible independence. In the preface to Part II. of "A New System of Chemical Philosophy" (1810), he declares:

> Having been in my progress so often misled, by taking for granted the results of others, I have determined to write as little as possible but what I can attest by my own experience. On this account, the following work will be found to contain more original facts and experiments, than any other of its size, on the elementary principles of chemistry.

Here the strong man places himself on record, and the question of priority takes to flight. Accordingly, I state it as my clear impression that the merits and defects of his achievement are alike traceable to the fact that our laureate lay under direct obligation to but one of his

[21] His chief work is, "Experiments and Observations relating to Acetous Acid, Fixable Air, Dense Inflammable Air, Oils and Fuel, etc." (1786).

[22] "A Comparative View of the Phlogistic and Antiphlogistic Theories, with Inductions, etc."

[23] P. 132 (2d ed., 1791).

predecessors—Newton. Dalton encountered certain phenomena, such as multiple and definite proportion, aqueous vapor as a distinct constituent of air, and, seeking for the simplest common representation, found it in Newton's well-known doctrine. For example, he says:

> According to this view of the subject [heat], every atom has an atmosphere of heat around it, in the same manner as the earth or any other planet has its atmosphere of air surrounding it, which can not certainly be said to be held by chemical affinity, but by a species of attraction of a very different kind.[24]

And he quotes from Newton:

> All bodies seem to be composed of hard particles. . . . Even the rays of light seem to be *hard* bodies, and how such very hard particles which are only laid together and touch only in a few points, can stick together, and that so firmly as they do, without the assistance of something which causes them to be attracted or pressed towards one another, is very difficult to conceive.[25]

This was the secret of the opposition of Hope and, later, of Faraday's complaint. In a letter, dated January 2, 1811, Hope wrote to Dalton as follows:

> I need not conceal from you that I am by no means a convert to your doctrine, and do not approve of putting the result of speculative reasoning as experiment.

While Faraday, similarly suspicious, as late as 1844, said:

> The word atom, which can never be used without involving much that is purely hypothetical, is often *intended* to be used to express a simple fact. . . . There can be no doubt that the words definite proportions, equivalents, primes, etc., . . . did not express the hypothesis as well as the fact.[26]

The truth is that Dalton was a first-rate theorist, who arrived at his conclusions, not primarily on the basis of induction from experiment, but by reflection. Analogically, he imports the view of "matter" peculiar to celestial mechanics, through molecular physics, into the realm of chemistry. Proceeding thus deductively, he evinces little awareness of the very complex problems involved, which the later developments of the atomic theory were to reveal. Cut off from the world, he did not possess intimate acquaintance in detail with the labors of his immediate predecessors and contemporaries—a happy accident, no doubt. For, this freedom from puzzle and disturbance enabled him to proceed boldly with a generalization when men of the caliber of Wollaston and Davy hung back. Dalton had natural capacity for logical thought, and complete confidence in the validity of those mathematical syntheses of *physical* facts which he had pondered.

But, as happens frequently, his limitations are traceable to the same source. Like Kant before him, Dalton became so entangled in the theoretical ways of his own thought that, after he had promulgated his

[24] *Manchester Memoirs*, Vol. II. (2d series), pp. 287 f.

[25] Royal Institution Lecture Notes.

[26] "Experimental Researches," Vol. II., pp. 285 f.

theory, he stopped short in middle life, and could not appreciate the work of others who followed and supported him. This is the blot on his 'scutcheon. Still, even so, we must hold the balance true. The kinetic doctrine of "matter," integral to the Cartesian philosophy, had paled before Newtonian atomism. And Dalton had grasped Newton's view so logically that he could not admit the law of equal volumes, because, as he held, "no two elastic bodies agree in the size of their particles." The very success of his hypothesis blinded him to Gay-Lussac's experimental evidence—it would not conform to the conceptual scheme. As he wrote to Berzelius, in September, 1812:

> The French doctrine of *equal measures* of gases combining, etc., is what I do not admit, *understanding it in a mathematical sense.* At the same time I acknowledge there is something wonderful in the frequency of the approximation.[24]

Of course, the fact was that, as Wurz points out,

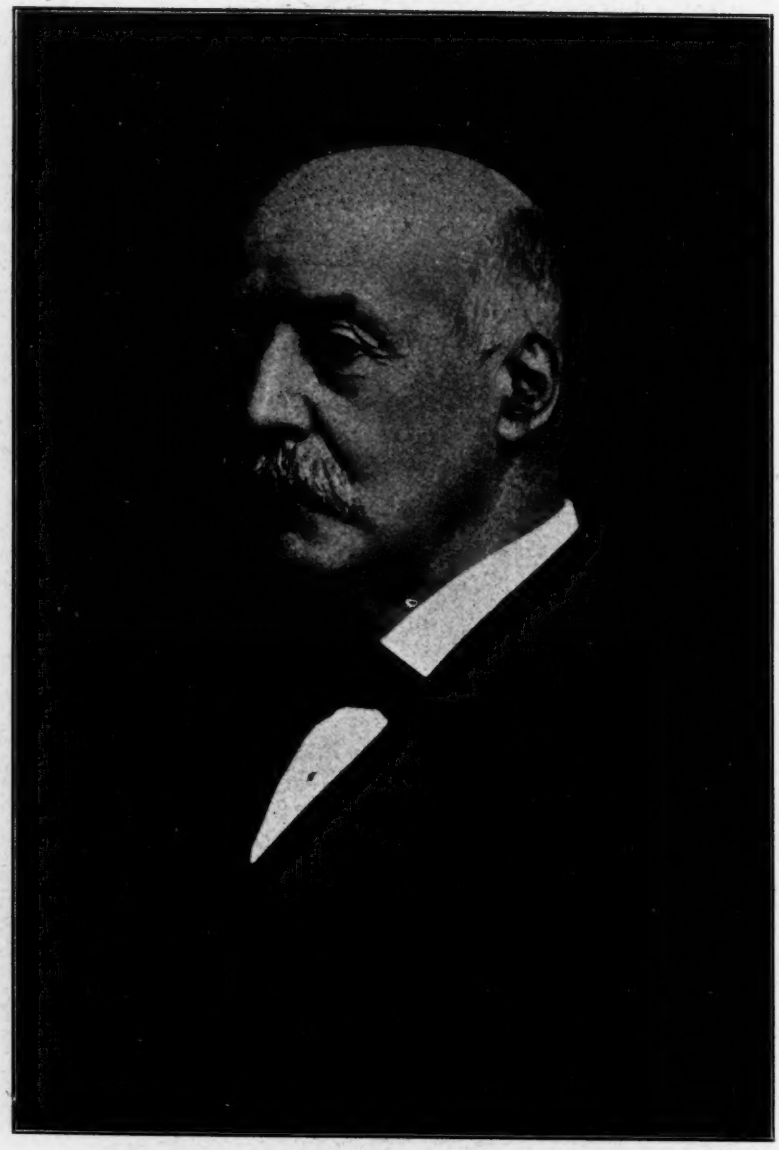
> The relation which exists between the densities of gases and their atomic weights is not so simple as we should at first sight be led to expect, and as for a long time it was thought to be.[25]

Nay, "understanding it in a mathematical sense," Dalton had his reasons. By a kind of paradox, the very simplicity of his notion befogged him here, just as the problems bred of the atomic theory diverted chemists for many a long day from the study of affinity.

We may conclude, then, that the logical character of Dalton's mind enabled him to formulate the timely conceptual representation on which chemical logic has pivoted ever since; that his numerical conception has stood the test of further discovery better than most hypotheses; and that, little as he knew it, or could admit it at the moment, he laid the foundation for that intimate alliance between physics and chemistry which forms one of the most pregnant among contemporary movements. For, the active criticism of the atomic theory—that it dogmatizes about the physical constants marking the differences between the elements, that it reveals little or nothing of the *processes* incident to chemical composition and destruction, that it neglects synthesis—testifies also, if negatively, to the revolution wrought by its author. Pity is akin to praise here. And to-night, as we celebrate Dalton's "thoughts that breathe," we are bound to let praise have its free way, especially when we contemplate the indomitable devotion of a character who, amid sore difficulties, but furnished with the splendid spur of consecration to the ideal, achieved so much for man's conquest of the secrets of nature.

[24] The italics "understanding," etc., are mine.

[25] "The Atomic Theory," p. 35 (English translation).



ALEXANDER AGASSIZ.

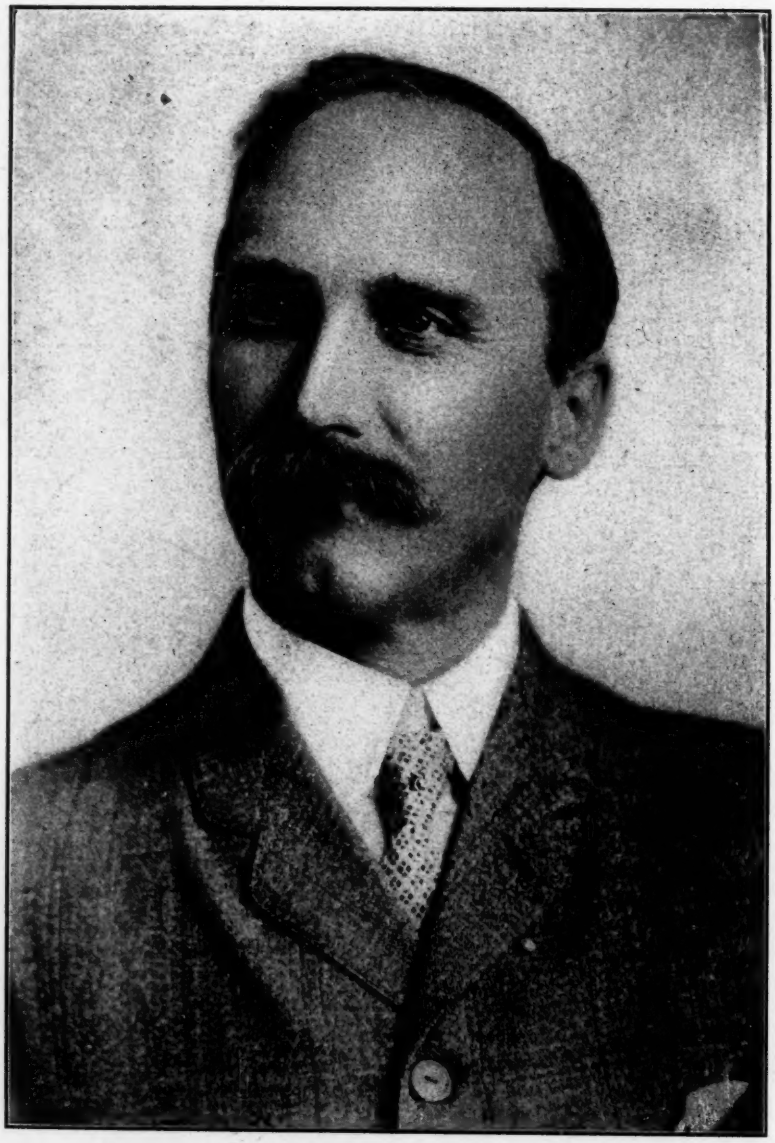
THE PROGRESS OF SCIENCE

THE DEATH OF ALEXANDER AGASSIZ

In the death of Alexander Agassiz, America loses its foremost naturalist, as a few months ago in the death of Simon Newcomb it lost its most eminent representative of the exact sciences. Both were born in the year 1835, and in a century preeminent for science both gave distinction to this country when it was relatively backward in scientific productivity. Each maintained his intellectual leadership and continued his researches and publications to the very end of a long life. America is no longer behind the nations of Europe in the number of its scientific workers, but among them all are none to take the places left vacant by Agassiz and Newcomb.

Alexander Agassiz was endowed at birth with the heritage of his great father, Louis Agassiz, whose work at Harvard he carried forward. Born in Switzerland, he came to the United States in 1849 at the age of fourteen and graduated from Harvard College in 1855, continuing graduate studies in mining and chemistry in the Lawrence Scientific School. In 1859 he went to California as an assistant on the coast survey and in the following year became assistant in the museum founded by Louis Agassiz, during whose absence in Brazil he was in charge. From 1866 to 1869 he was engaged in mining in the Lake Superior region and became superintendent of the Calumet and Hecla copper mines of which he was president at the time of his death. He thus acquired abundant wealth, and was able to give more than half a million dollars to the Harvard Museum of Comparative Zoology and to conduct as he wished his oceanographical expeditions.

In 1869 Mr. Agassiz visited European museums and on his return in 1870 renewed his duties at the Harvard Museum, of which he became curator and director on the death of Louis Agassiz in 1873. He was for a series of years one of the seven fellows who form the corporation of Harvard College, and was on two occasions elected an overseer. In 1875 he visited the western coast of South America and subsequently went to England to assist with the reports of the *Challenger* expedition, writing the monograph on the Echini. Previously and subsequently to the end of his life, he made a great number of valuable scientific contributions to marine zoology, the embryology of fishes and coral reefs. In awarding to him its Victoria research medal, the report of the Royal Geographical Society said "he has done more for oceanographical research than any other single individual" and summed up his work by noting that for thirty years he had carried out personally oceanographical expeditions over most of the oceans of the world. In 1877–80 he explored the Florida Straits and Gulf of Mexico, the Atlantic Coast and the Caribbean Sea. In 1880 he studied the surface fauna of the Gulf Stream; in 1892–4 he investigated the Sandwich Islands, studying recent and extinct reefs. In 1891 he conducted three cruises off the West Coast of Central America, and in 1895–6 he studied the Great Barrier Reef of Australia and in 1897–8 the Fiji Islands. In 1899–1900 he carried out a cruise from San Francisco via the Coral Island groups to Japan. In 1904–5 he investigated the eastern tropical Pacific. In the Indian Ocean in 1901–2 he devoted himself to the Maldive Islands. In 1874–5 he investi-



CHARLES REID BARNES.

gated Lake Titicaca. Mr. Agassiz has done this entirely at his own expense. The results have been published by him through the Museum of Comparative Zoology at Harvard College, in thirty volumes of memoirs and fifty-three volumes of bulletins, mostly containing the results of his own various expeditions and of the work of the specialists who examined his collections. Besides the numerous publications through the Harvard Museum, in 1888 Mr. Agassiz published in two volumes the narrative of his three cruises in the Gulf of Mexico, the Caribbean Sea, and along the Atlantic Coast of the United States, with charts and illustrations.

Mr. Agassiz had been president of the National Academy of Sciences, and was a foreign member of the leading academies of the world. He was not only the author of important contributions to science, but was also a great man, possessed of complete courage and frankness and a dominant will, which gave him leadership throughout the broad and rich experiences of his long life. As he was happy in his birth and in his life, it may be said that he was not ill-starred in his death, for he died with faculties undimmed, suddenly, on the sea, which he loved so well and had explored so persistently.

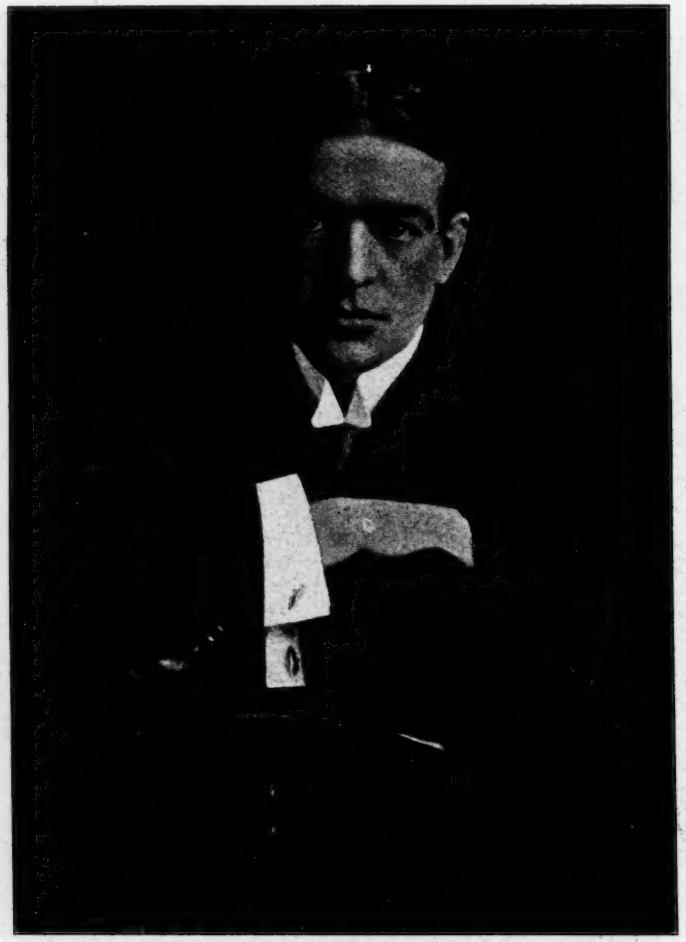
CHARLES REID BARNES

THE death of Dr. Charles Reid Barnes, professor of plant physiology at the University of Chicago, as the result of a fall, is a serious loss to botany. He was born at Madison, Ind., in 1858 and was educated at Hanover College and Harvard University. After occupying successively the chairs of natural history and of botany and geology at Purdue University, he was called to the chair of botany at the University of Wisconsin in 1887, where he remained until he took up his final work at Chicago in 1898. During all of these years he was associated with Professor Coulter in the editorship of the *Botanical Gazette*. Professor Barnes had served as vice-president of the botanical section of the American Association and as president of the Botanical Society of America. Professor Barnes's best known earlier publications dealt with the taxonomy of mosses. Just before his death he completed the final proof-reading of the physiological part of a general textbook of botany that is expected soon to appear from the Hull Botanical Laboratory. Within the past few years Professor Barnes had become greatly interested in morphological problems among the bryophytes, two papers having been already published in conjunction with Dr. Land and several others being partly ready.

THE TROUBLES AT PRINCETON

THE secret history of almost any American university is not less complicated than recent events at Princeton, but it is certainly unusual for such family quarrels to be so completely exploited before a public which can scarcely be expected to understand them. It is, however, probably not a bad thing for a university to conduct its affairs in the open and for large numbers to become interested in them, even though the principles involved may not be so vital as they appear to those immediately concerned. Probably the circumstance of greatest general interest at Princeton is the control exercised by the alumni. This is a factor likely to become increasingly important in the history of our universities, and it is not without its dangers, for the alumni bear gifts and are more likely to be concerned with athletics and fraternities than with scholarship.

The outlines of the Princeton story are now common property. Dean West has long urged with enthusiasm a graduate college on the lines of the Oxford colleges and President Wilson approved the plan. Then came President Wilson's move against the clubs—fraternities are forbidden at Princeton—and in favor of more democratic "quads," which divided the faculty and trustees and awakened the opposition



LIEUTENANT ERNEST SHACKLETON,
the eminent arctic explorer, who is at present lecturing in the United States.

of the rich alumni. The Swan bequest of $300,000 for a graduate college then became available, and there was difference of opinion as to its site. Mr. W. C. Proctor at this stage offered to give $500,000 for the graduate college as planned by Dean West and on condition that an equal sum should be subscribed by others. There was again difference of opinion as to the site and the control of the college, and while the president and a committee of the trustees were trying to come to an agreement with Mr. Proctor, he withdrew his gift.

The question of site is somewhat trivial except in so far as it has become identified with policies. Whether the residence hall should be in the midst of the Princeton campus or on its outskirts can not be a matter of serious consequence. The fact is that the president of the university and some of the trustees were unwilling to place the dean of the graduate school in as complete control of its development as the acceptance of Mr. Proctor's gift might have implied. The real trouble is one of men rather than of measures.

It is a curious circumstance that President Wilson and Dean West are in pretty close agreement in favor of a financial democracy and of an intellectual aristocracy or snobbishness, as one may please to call it. When Dean West favors a residential college with oak-panelled dining hall in which the students shall dine in evening dress, he does so because he wishes to give the young men without money a chance to live in the environment which he regards as proper to the scholar and the gentleman. The ideal of such a college was well put in an address made some years ago. We read of

> a place removed—calm Science seated there, recluse, ascetic, like a nun, not knowing that the world passes, not caring, if the truth but come in answer to her prayer; and Literature, walking within her open doors, in quiet chambers, with men of olden time, storied walls about her, and calm voices infinitely sweet; here "magic casements, opening on the foam of perilous seas, in fairy lands forlorn," to which you may withdraw and use your youth for pleasure.

Those who have followed the recent Princeton controversy may be surprised to learn that this not a quotation from Dean West, but from the concluding part of Dr. Wilson's address on the occasion of the Princeton sesquicentennial celebration. It might be that President Wilson had learned new things in the meanwhile, but at the meeting of the Association of American Universities a couple of months ago, he presented a paper urging the old ideas of amateurism and dilletantism in college studies. He writes:

> All specialism—and this includes professional training—is clearly individualistic in its object; that is, the object of professional training is the private object of the person who is seeking that training. . . . The minute professionalism enters learning, it ceases to wear the broad and genial face of learning. It has become a commodity; it has become something that a man wishes to exchange for means of support. It has become something that a man wishes to use in order to get the better of his fellowmen; to enhance his fortunes; to do all the things that center in and upon himself; and it is professionalism that spoils the game, the game of life, the game of humanity, the game of cooperation in social undertaking, the whole handsome game that we are seeking to throw light upon by the processes of education.

It is a remarkable and interesting fact that Princeton is becoming a great university and a great scientific center almost in spite of those in control. The large gifts made to the university have found their way to build fine laboratories and to secure scientific men of the first rank. The preceptors intended for less modern purposes brought to Princeton a large group of younger men from various institutions who have given it new life. The efforts for a graduate residential college, which in Dean West's words should "show that God is the end of all our knowing and

Christ is the Master of the Schools," or, in President Wilson's phrase, should be "quick to look toward heaven for the confirmation of its hope," will lead to a true graduate school for the training of professional scholars and the advancement of knowledge.

SCIENTIFIC ITEMS

WE regret to record the deaths of Professor Robert Parr Whitfield, curator of geology of the American Museum of Natural History; Dr. Borden Parker Bowne, professor of philosophy at Boston University, and of Dr. Eduard Pflüger, the eminent German physiologist.

DR. T. MUIR, F.R.S., has been elected president of the South African Association for the Advancement of Science for the meeting in Cape Town, the date of which is not yet set.—Dr. George W. Hill, of Nyack, N. Y., and Professor E. B. Wilson, of Columbia University, have been elected foreign members of the Brussels Academy of Sciences.—A testimonial dinner to Dr. Charles Frederick Chandler was given at the Waldorf-Astoria on April 2, to permit his former students and associates to express, before his retirement, their appreciation of his forty-six years of service to Columbia University, and his lifetime of devotion to the cause of education and science. It was announced that a lectureship in honor of Dr. Chandler would be endowed by his former students and that the chemical museum of the university would be named in his honor.

THE Oceanographical Museum at Monaco, established by the Prince of Monaco, was opened on March 29. The different European governments and the principal scientific societies were represented at the ceremony.—A Brooklyn Botanic Garden is now being established by the City of Greater New York in cooperation with the Brooklyn Institute of Arts and Sciences. Between twenty-five and thirty acres of land, south of the museum building of the institute in Brooklyn, have been set apart for the purposes of the garden. A laboratory building for purposes of investigation and instruction, together with a range of experimental and public greenhouses, will be constructed during the coming summer and autumn. For this purpose the City of New York has appropriated $100,000 and friends of the garden in Brooklyn have subscribed $50,000 as an endowment. Dr. C. Stuart Gager, professor of botany in the University of Missouri, has been appointed director.